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(FOUNDED 1897 in succession to the ANNUAL REPORTS)

Edited for the Council by J. LAURENCE PRITCHARD, Fellow

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NOTICES

Elections

The following Members were elected at a Council Meeting held on March 10th:—

Fellow.—Mr. A. H. R. Fedden.

Associate Fellows.—Mr. B. L. Martin and Captain F. W. Merriam.

Wilbur Wright Lecture

The Council have decided that the Wilbur Wright Lecture shall be held this year in the Library at 8.30 p.m., on Thursday, April 30th, when Rear-Admiral D. W. Taylor will read a paper on "Some Aspects of the Comparison of Model and Full-Scale Tests."

Dutch Lecture

A joint meeting of the Royal Society of Arts, the Royal Aeronautical Society and the Anglo-Batavian Society will be held at the Royal Society of Arts, on Wednesday, May 13th, at 8.0 p.m., when a paper will be read on the recent flight from Holland to the Dutch East Indies. Air Vice-Marshal Sir Sefton Brancker will take the chair.

Secretaryship

In reply to enquiries the Council desire to state that it is their present intention to make the post of Secretary an honorary one, when the present Secretary vacates the appointment on May 1st.

Lantern Slides

The Council desire to thank the following firms for donations to the Society's loan collection of lantern slides:—A. V. Roe and Co., Ltd., The Bristol Aeroplane Company, Ltd., The de Havilland Co., Ltd., The English Electric Company, Ltd., The Gloucestershire Aircraft Co., Ltd., D. Napier and Son, Ltd., Rolls-Royce, Ltd., Vickers, Ltd., and The Westland Aircraft Works.

Lecture

It is regretted that Colonel Frank Searle has found it necessary to cancel his lecture on "The Maintenance of Commercial Aircraft," arranged for April 23rd.

Forthcoming Arrangements

Tuesday, April 21st, 5.30 p.m.—Council Meeting.

Thursday, April 30th, **8.30 p.m.**—In the Library, Wilbur Wright Lecture, Rear-Admiral D. W. Taylor, "Some Aspects of the Comparison of Model and Full-Scale Tests."

W. LOCKWOOD MARSH, *Secretary.*

PROCEEDINGS

First Meeting, Second Half, 60th Session

A meeting of the Royal Aeronautical Society was held in the Society's Library at 7, Albemarle Street, London, on Thursday, January 22nd, 1925, when a paper on "Some Recent Work of the Aerodynamics Department, National Physical Laboratory," was read by Major R. V. Southwell, Associate Fellow.

Mr. W. O. Manning presided in the absence of the Chairman of the Society, who was abroad.

SOME RECENT WORK OF THE AERODYNAMICS DEPARTMENT, NATIONAL PHYSICAL LABORATORY

BY R. V. SOUTHWELL, ASSOCIATE FELLOW.

1. No one who has the honour to serve on the Council of this Society, and can therefore be tackled in person once in every month, can long avoid the additional but more alarming honour of being called upon to lecture. When I in my turn was brought to bay, I cast about for a suitable subject, but could not satisfy myself that any of the work in which I happen personally to be engaged had a sufficiently general appeal. I therefore decided to take advantage of my association with the Aerodynamics Department of the National Physical Laboratory, and to base my lecture on the work of other men. I want to make this clear at the outset, because I may omit in some instances to attribute credit where credit is due, and at all events it must be understood that the credit does not belong to me.

I decided that my lecture should be a review of work which has been done at Teddington during my period of service there,—that is, since the summer of 1920. By expressly restricting my survey to our own work, I hoped as it were to sidestep strictures which I should otherwise invite by my ignorance of work done abroad; and the period I chose, not because I regarded my arrival as starting an epoch in the history of aerodynamics, but because I should thereby deal with researches which at all events I saw and criticised during their progress. This period, moreover, has a significance of its own, because in effect it is the "post-war" period in aerodynamic research. When a change of policy occurs in high quarters, it is not for some time that its consequences are apparent; and the new outlook which naturally resulted from the cessation of war, and hence of urgent demands for experiments of an *ad hoc* nature, did not much affect the programmes of our research establishments before the year with which my survey begins.

2. There is, I think, some advantage to be had by occasionally taking stock in this way of progress made within a given period. The slow rise of a building is not most clearly seen by those who are at work upon the walls, and some of us are at times inclined, by our very desire for the advancement of aeronautical science, to take a despondent view of the progress which it is making. Such discontent, in an imperfect world, is inevitable and indeed healthy; but it can do us no harm to realise, on standing further away from our subject, that progress does in fact occur, and I myself have found comfort in my study of the Annual Reports which have been presented since 1919. I went back to the Report for that year, because it reviewed the whole of the work done in the war period, much of which

had previously been kept secret; and despite the intense activity of those five years, I have no hesitation in saying that progress has been swifter in the five years which have followed them. If I am asked the reason for this, I would say: "the revival in this country of interest in the theoretical aspects of aerodynamics, and in particular, the spread of belief in the underlying concepts of the Lanchester-Prandtl theory."

I do not want to give the impression that research is tending away from experiment and towards theory; rather, as it seems to me, we are tending to plan our experiments more and more in the light of theory, which means that the results, when obtained, fall more and more into an ordered scheme. To illustrate my point, let me now briefly summarise the advances which have been made during the period 1920-1924 in the main fields of aerodynamic inquiry. My concern tonight is with methods rather than with results, of which some have been given you already by Prof. Jones, Mr. Farren and others, and more will without doubt be given in future lectures of this Society.

I should explain, in the first place, the range of our activities at Teddington. By far the greater part of our work is conducted for the Air Ministry and controlled by the Aeronautical Research Committee, but we carry out each year extensive researches, both on model and full scale, for the Admiralty and War Office, and a number of minor investigations for Research Associations or private firms. Such work is almost always of a confidential nature, and I am not at liberty to describe it here. But although my survey is restricted to work done for the Aeronautical Research Committee, it is not confined to problems in aerodynamics; our representation on Committees brings us problems of other kinds—notably in the theory of elasticity and in the economics of air transport—and some of these I shall mention.

Work on Stability and Control

3. Problems of stability and control have long been placed by the A.R.C. in the forefront of its programme, and I begin with these. In 1920, the Duplex Wind Tunnel, and its balances of novel design, were nearing completion; suspension of models in the wind tunnel by wires rather than on the bottom balance was becoming standard practice in an increasing number of experiments; rotary derivatives were still measured by observing the rate of decay of free oscillations when the motion of the model was controlled by springs. In addition to experiments in continuation of work started previously, measurements of lift, drag and pitching moment, and of the derivative M_q (the damping coefficient for pitching motion) were made on six complete aeroplane models which, at the instigation of Prof. Bairstow, had been specially ordered by the Air Ministry for the purpose of systematic research.

4. In 1921 the Duplex Tunnel was in full operation, although the special 1/5th scale model aeroplane, fitted with its own electric motor, was not yet ready. Mr. Relf used this tunnel for his pioneer work on the measurement of lateral stability derivatives. He discarded the method of free oscillations for one in which the model was given a forced harmonic motion by an eccentric acting through a spring, and he determined derivatives of the type L_p (the damping coefficient for rolling motion) from the conditions required for "resonance". Fig. 1 shows the arrangement in the wind tunnel. For measuring "cross-derivatives" (*e.g.*, the damping in roll which results from a motion in yaw) he employed an even more ingenious method. The model, as you will see, is so held that motion in yaw can be imposed by an eccentric gear fitted to the electric motor without introducing any constraint of the rolling motion: Mr. Relf varied the speed of the eccentric, not this time until the amplitude of the motion was a maximum, *but until the rolling and yawing oscillations were in phase,* and he employed theory to derive a relation, obtaining under these conditions, between the "cross-derivative"

whose value was required and a "direct derivative" (damping in yaw due to motion in yaw) which had been measured previously. The attainment of the required condition in the experiment was recorded by a spot of light reflected from a mirror on the model: when the two motions are in phase, the path of this spot degenerates from an ellipse to a straight line of which the *slope* alone is significant.

5. These measurements, in 1921, extended up to, but not beyond, the critical angle of incidence. In 1922 the necessary extension was made, with complete success, by introducing electro-magnetic damping which could be adjusted to suit the requirements of any particular experiment; and since in the meanwhile special apparatus had been constructed for the determination of the non-rotary derivatives at any required presentation of the model, complete data in regard to lateral

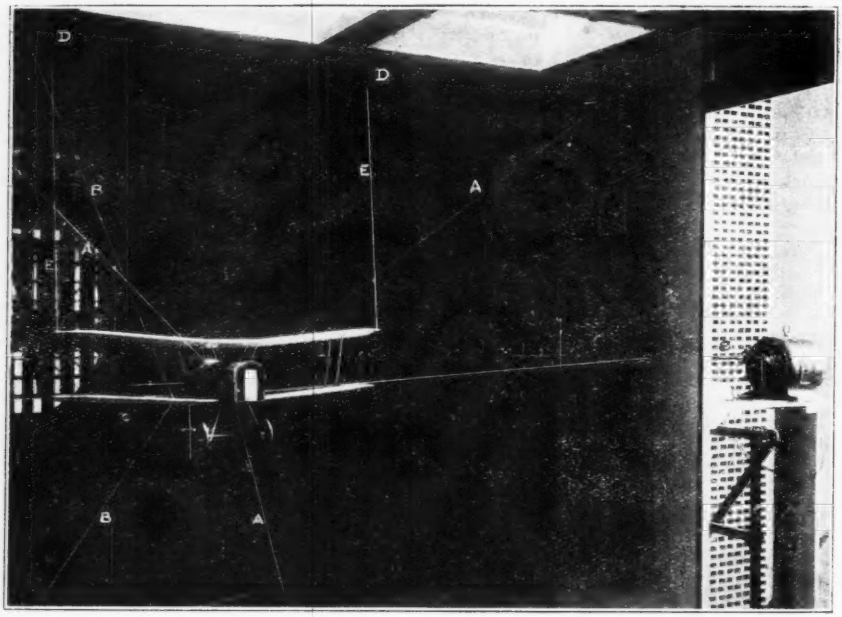


FIG. 1.

stability characteristics were available for the first time. The model employed in these tests was the S.E.5A, and the resulting data were analysed by Mr. Relf on the lines of the classical stability theory.

I ought in passing to describe the "three force and three moment" apparatus for non-rotary derivatives, although I included it in my paper to the International Air Congress of 1923. A tiny double-gimbal ring, with hard steel points as pivots, is inserted in the body of the model, and employed to support the model on a spindle held in the bottom balance. The model is thus free to take up any angular position,—*i.e.*, it has three degrees of freedom. A wire is led from a light balance arm, specially fitted to the main balance, to a point near one wing tip, and from a corresponding point on the opposite wing tip a weight is suspended (Fig. 2). In this way the roll of the model is constrained, and the rolling moment can be obtained by direct weighing with wind "on" and "off". A similar arrangement

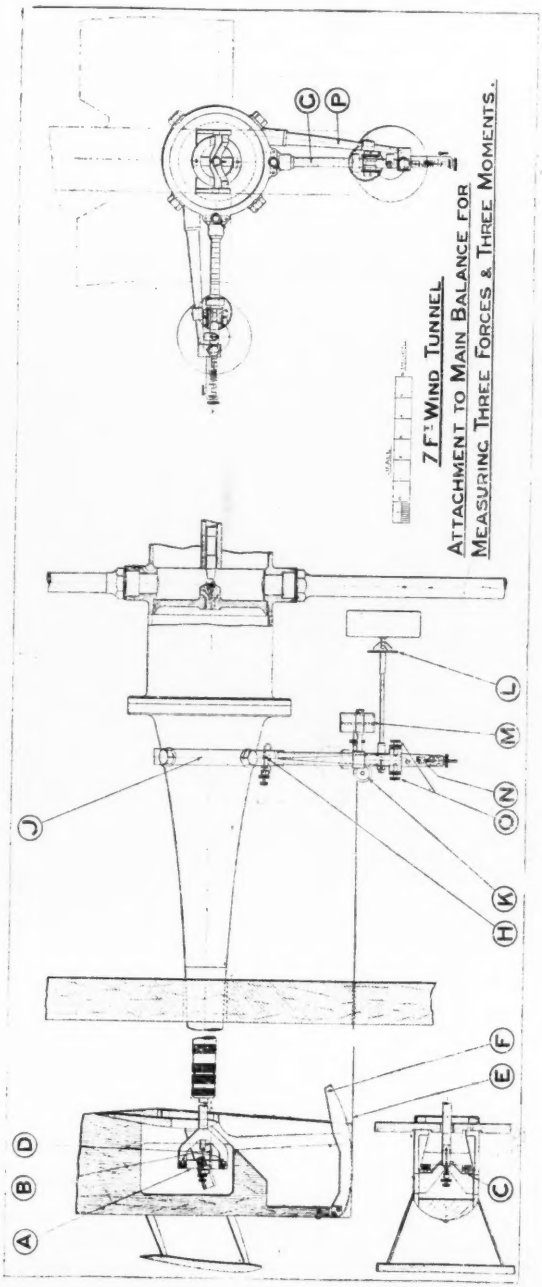

7 Ft Wind Tunnel
Attachment to Main Balance for
Measuring Three Forces & Three Moments.
C
P
L
M
N
O
J
K
H
A
B
D
E
F
C

FIG. 2.

provides for the measurement of pitching-moment, and when the two light balance arms are clamped the model is rigidly fixed to the main balance, which then measures the remaining couple (in yaw) and the three forces on the model. Adjustment in pitch and roll is effected by paying out the attachment wires from small windlasses on the light balance arms, and adjustment in yaw by means of the balance turntable.

6. In Mr. Relf's oscillation experiments above the stall an unexpected result was obtained: the derivative L_p (damping in roll) did not change sign, as the phenomenon of autorotation had suggested that it would do. This conclusion was so surprising that it could hardly be accepted without confirmation, and a

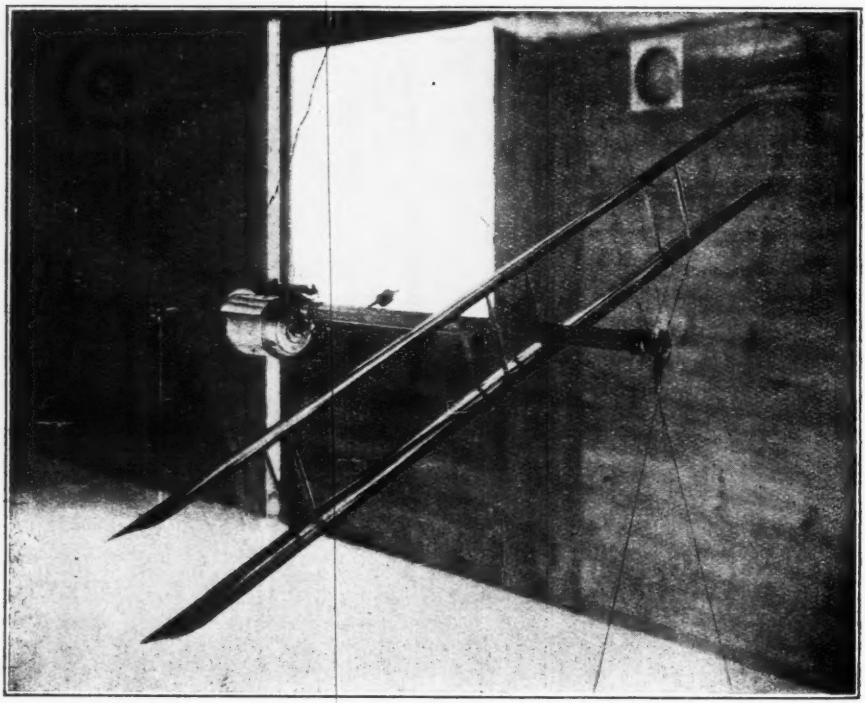


FIG. 3.

"continuous rotation balance" was accordingly constructed, on lines first suggested by Mr. McKinnon Wood, but capable of very slow running: Fig. 3 shows the apparatus in tunnel. It was found that L_p did in fact retain its sign above the stall, in the S.E.5A model, when the rotation was very slow, although at higher speeds the moment changed sign and autorotation set in.

7. In 1923-24, oscillation experiments by the methods established by Mr. Relf were carried out by Mr. Frazer and others on a Bristol Fighter model, and the non-rotary derivatives for this model were measured on the "three-force and three-moment apparatus" by Messrs. Irving and Batson, who had done the previous work on the S.E.5A. We thus have at the present time complete model data in regard to lateral stability for two standard machines. But these further experiments indicated that many difficulties, in general, attend the measurement of

rotary derivatives by oscillation methods at angles of presentation above the "stalling angle". This is hardly surprising if one attempts to picture what is happening, and it suggests that "continuous rotation" methods will be preferable for such work.

In a wind tunnel the only continuous motion which is possible is a motion in roll. We have recently modified our "continuous rotation apparatus" to permit the measurement of all three moments introduced by roll, and thus we have reached a limit in this direction. What we want now is similar measurements for a model which has a steady angular velocity in pitch or yaw, and evidently recourse must be had to the whirling arm. The arm which exists at Teddington is hardly suitable, so we have recently designed an entirely new gantry which is now under construction by contractors, and should be in operation by next summer. The strength is in a double cantilever arm, but by means of a kind of glorified "Meccano outfit" a cubical bay of 7-foot side (to conform with wind-tunnel dimensions) can be attached at any of three different radii to support the model and measuring apparatus.

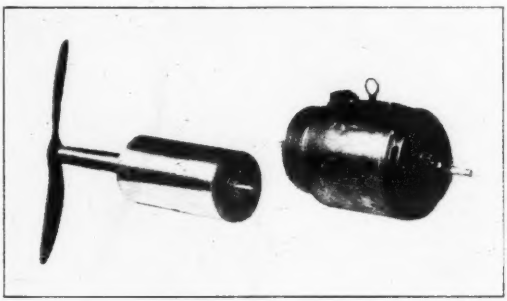


FIG. 4.

8. Towards the close of last year Mr. Relf completed his measurements of performance characteristics for our large Bristol Fighter model in the Duplex Tunnel, and began work on the measurement of stability derivatives. We shall shortly present the results as regards the derivative M_q (damping of motion in pitch), and then the more difficult problem of measuring lateral derivatives on this model will have to be attacked. Here I must leave my survey of work on stability; *we have just reached the stage at which, for the first time, stability data with allowance for slip-stream effects can be obtained on a model.* As a matter of interest I show a slide (Fig. 4) of the small three-phase motor (designed by Mr. Relf, and constructed in our own workshops) which has made this possible; although only six inches in diameter, it is capable of developing, for short spells, very nearly 3 h.p. I also show (Fig. 5) the arrangement of the model in tunnel for the measurement of M_q—an excellent example of present-day technique.

Performance

9. Much other work has been done, on problems of stability and control, of which I have not time to speak to-night. Nor, as I have already explained, is it in the scheme of this lecture to discuss results; some of these have been given you recently by Prof. Jones in his lecture on the problem of Low Speed Control.

For a similar reason, I shall only touch on problems of performance. Most of our work in this direction has been aimed at the elucidation of scale-effect, and Mr. Farren's lecture on the recently published Scale-Effect Report of the Design

Panel will be fresh in your minds; some has been undertaken to elucidate and test the underlying assumptions of the Prandtl theory, and of this I shall speak later. From the standpoint of experimental method one may say, speaking generally, that performance problems make less demand than problems of stability on wind-tunnel technique; it also appears that the methods are approaching finality, wire suspension being invariably adopted.

Airscrews

10. In 1920 a very important research on airscrews was brought to a satisfactory conclusion by Mr. Fage, namely, a determination of the pressure distri-

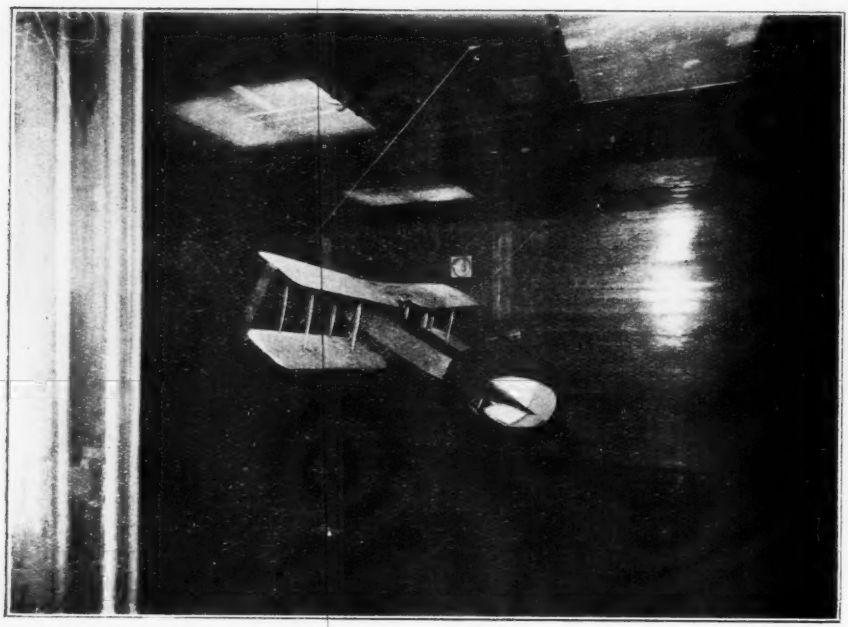


FIG. 5.

bution over the entire surface of the rotating blade. This was followed in 1921 by a very lengthy series of experiments on a "Family of Airscrews", in which, starting from an approved design for plan form, blade sections, etc., such variables as pitch-diameter ratio, number of blades and blade-width were systematically varied. The work was initiated by the Airscrew Panel of the A.R.C., and was brought to completion in 1922; that was the year in which Mr. Glauert's paper on the Vortex Theory of Airscrews was presented, and from that time onwards its influence has been noticeable in practically every airscrew report. Indeed, most recent work has been aimed at testing the consequences of the theory, and of a semi-empirical extension which has recently been proposed by Mr. Lock to cover the whole working range of an airscrew; quite recently, the pressure-plotting experiments of 1920 have been analysed afresh on its basis.

Here perhaps is my best example in support of a remark which I made when I started, namely, the dominating influence of the Lanchester-Prandtl theory, which is of course the foundation of Mr. Glauert's work. One well-known designer has

even given it as his opinion that the vortex theory of airscrews is a more satisfactory basis for design than the empirical data of the "Family of Airscrews" research. However that may be, I think every member of the Airscrew Panel would agree that its scheme for the experiments would have been considerably modified had the vortex theory been available when it was drawn up.

Airships

II. 1920 and 1921 were years of great activity in full-scale research on airships, which has always come within the province of the N.P.L. In August, 1921, the lamentable accident which befell R.38 in her trial flight resulted in the death of

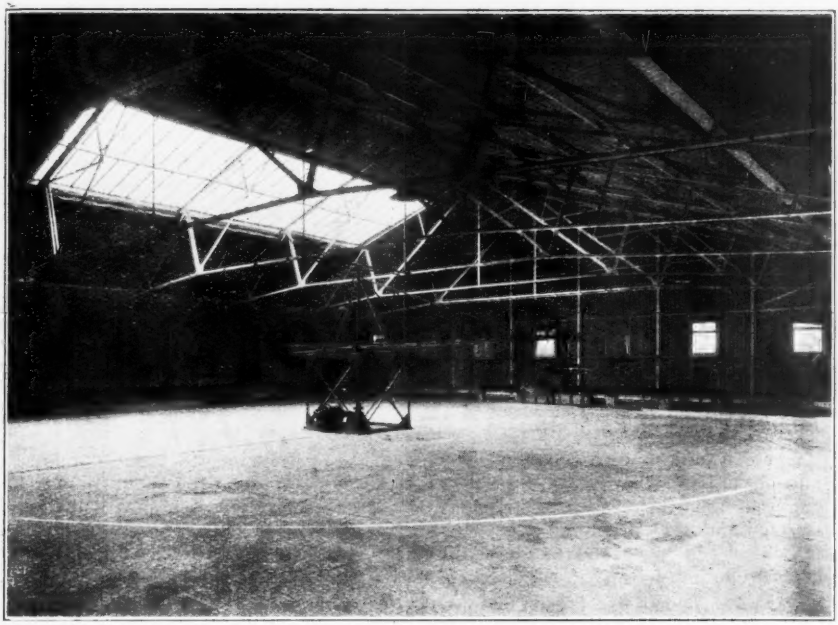


FIG. 6.

two members of our Department, Mr. J. R. Pannell and Mr. C. W. Duffield; another member, Mr. H. Bateman, was among the few survivors. This disaster, as is well-known, resulted in a complete discontinuance of airship construction in this country; much to our regret, it also led to the discontinuance of experimental work, even on models.

That this was a grave error of policy is, I think, apparent to all who are now endeavouring once more to establish airship design and construction on sound lines: data are needed, and needed in a great hurry, which would have been forthcoming naturally in the last three years if we had been free to proceed. We did what we could: we initiated a very complete determination of the pressure distribution on a prolate spheroid, both in rectilinear motion (by wind-tunnel experiments), when turning about a minor axis, and when moving with a combination of translational and rotational motion (on the whirling arm). Since this was a mathematical shape, we were not doing airship experiments, and were therefore "in order"; a comparison with theory could be made (and has given very satis-

factory results); and the shape does in fact resemble an airship very closely, except near the tail, where the flow is in any case seriously disturbed by the fins. Fig. 6 shows the spheroid on the whirling arm. Presentation of the results has been delayed by the unfortunate illness of Dr. R. Jones, who has employed them, with others from earlier experiments, to develop a theory of the turning airship which has been found of great value in the last few months.

12. The Department has also endeavoured to assist in the stressing problems which are associated with airships, by means of theoretical investigations of braced tubular frameworks to which the hulls of actual airships approximate. The papers which it has published have been used to a considerable extent in the preparation of the Report of the Airship Stressing Panel, which was appointed by the Air Ministry in December 1921, with terms of reference which are indicated by its title. Now, with official interest in airships revived, model tests are again proceeding continuously, and we may hope that knowledge will grow without further check.

Economic Flight

13. Control and Stability, Performance, Airscrews and Airships,—these, with the secret researches to which I have referred, make up the list of our "regular" activities. They involve theoretical work, of course, as well as experimental,—for example, calculation of the motion of an aeroplane as affected by accidental disturbances or by movement of the controls. Since 1923 investigations have been proceeding without interruption, for the Air Transport Sub-Committee of the A.R.C., on the economics of aeroplane flight, and Mr. Cowley, who has charge of this work, has found many interesting directions in which it seems possible that economies might be effected. This subject has been recently treated by Mr. Tizard in a Wilbur Wright lecture, and I shall not pursue it here.

Fundamentals of Fluid Motion

14. I turn now to problems which fall less obviously into broad categories, and perhaps for that reason have a rather special attraction for the men who are engaged upon them. Most of the work which I shall describe originated within the Aerodynamics Department itself,—as of course was to be expected, because an attack on fundamentals cannot in the nature of things be closely controlled by a committee.

For aerodynamics I suppose no problem is so fundamental as the question—why does an aerofoil lift? We can hardly rest satisfied with the present position,—which is, that we have next to no idea. To answer the question completely would involve no less than the solution of the general equations of motion for a viscous fluid, and attacks on these equations have been made from all angles. Considering the energy expended, the results have been very small; but then, these are about the most intractable equations in the whole of mathematical physics, and the whole trouble of "scale-effect" is latent in their form. When the motion is very slow they can sometimes be solved. The labour is immense, if results of any generality are to be obtained; but in such matters you must take what you can get, and solve where and how you can. In this country we are indebted principally to Prof. Bairstow for the advances which have been made.

Messrs. Cowley and Levy proposed in 1920 a method of attack whereby a solution for the motion at high speeds was to be developed in successive steps from the solution for slow speeds, and Mr. Frazer has devoted such leisure as he could find in the last four years to a detailed investigation of the matter. His solutions bring out very clearly the extreme complexity of these problems, and they show, perhaps, that our chance of obtaining results by the method of Cowley and Levy is rather remote (Fig. 7).

STREAM LINES FOR TRANSLATING CYLINDER
The Curves plotted represent the System $\frac{\psi}{U}$ = constant. (Initial Term)
-125
-100
-75
-50
-25
-10
0
+10
+25
+50
+75
+100
+125
Outer circular boundary
148·4
Radius of Cylinder = 1·0
STREAM LINES FOR TRANSLATING CYLINDER
The Curves plotted represent the System $\frac{\psi}{U}$ = constant. (First two Terms)
Outer circular boundary
148·4
Radius of Cylinder = 1·0
STREAM LINES FOR TRANSLATING CYLINDER.
The curves plotted represent the system $\frac{\psi}{U} \times 10^{-81}$ = const. (First two Terms)
Outer circular boundary.
$26{\cdot}8 \times 10^{42}$
Radius of Cylinder = 1·0

FIG. 7.

15. At the other end of the scale we have problems in which the motion is so fast that the effects of viscosity are relatively negligible. A theory of such problems has been in existence for a long time; indeed, until recently it made up the bulk of hydrodynamical text-books. It involves the neglect of viscosity in this degree as well, that the fluid is assumed to slip freely over the solid boundary. Whether such solutions have any application to practice is a question for further investigation, since experiment shows that the fluid cannot slip in this way; and hence arises the need for a discussion of the "boundary layer" in Prandtl's Theory, as described by Prof. Bairstow in a recent lecture to this Society. One may say, I think, that the classical theory of an inviscid fluid, once discredited so far as concerns any possible application to real problems, is in way of becoming reinstated by modern work, as giving—*under proper control*, and aided by assumptions based on physical intuition—close approximations to the truth. The credit for this happy state of affairs is undoubtedly, I think, due to Prof. Prandtl and his school.

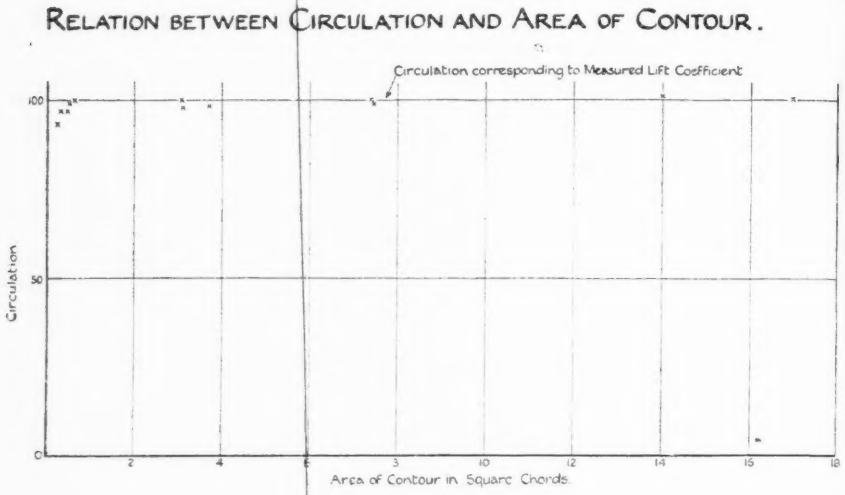


FIG. 8.

At the N.P.L. we have taken every opportunity to check the validity of the Prandtl theory, and in the main one must say, I think, that it has passed the ordeal with flying colours. Two kinds of tests are possible for a theory of this sort: either one can push its consequences to the utmost and check the resulting deductions, or one can test the underlying assumptions on which the theory is based. We have made tests of both kinds, but by far the most important are those which Messrs. Fage, Bryant, Simmons and Williams have made under the second head, in the form of complete explorations of airflow round aerofoils of infinite and of finite aspect ratio. The work is not yet published; but the most important result—the confirmation of the theoretical relation between lift-coefficient and circulation—has been described to you by Prof. Bairstow and others, and that I can show by one simple diagram (Fig. 8).

This proportionality between lift-coefficient and circulation is not the only assumption on which the Prandtl theory is based. The essence of the theory is the representation of lifting surfaces by appropriately chosen vortex filaments, and the treatment of the resulting motion by the classical theory of an inviscid

fluid; so the question immediately arises,—does the actual flow conform to the pattern which theory would predict? Now the calculation of the theoretical flow pattern for an arbitrarily specified aerofoil section is a very laborious matter, although the work of Prof. Bairstow, Major Low and others has provided us with the necessary technique; evidently it would be a great advantage to have a machine which will, as it were, grind out a sufficiently accurate solution when an aerofoil is placed in the slot, and this we now possess, thanks to Mr. Relf.

Fig. 9 shows the principle on which the apparatus works. It is well known that the hydrodynamic equations are mathematically identical with those which govern the distribution of potential in an electric field; hence, if we can deter-

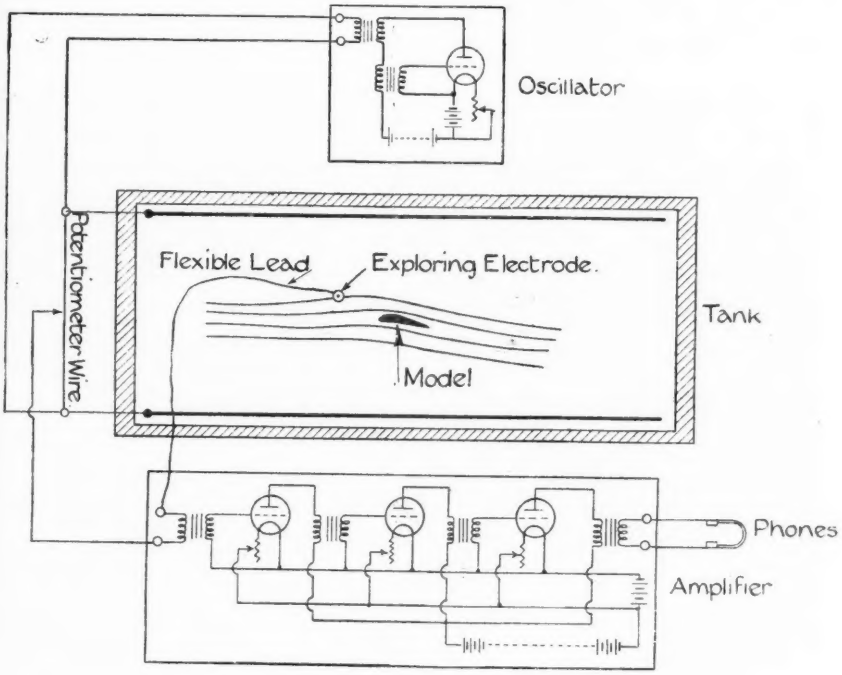


FIG. 9.

mine the potential in the field between two conducting plates, when a conductor of the specified shape is placed between them, we have determined the theoretical stream-lines for our aerofoil placed in a closed tunnel. Our apparatus explores a tank of water in which the conducting plates and the "aerofoil" conductor are placed. The method of operation needs little explanation; Fig. 10 shows the actual tank, and the pantograph apparatus for quick plotting of the results.

Eddy Motion

18. The assumption underlying Prandtl's theory is a *steady* motion approximating to a theoretical ideal; if *fluctuating* or eddying flow occurs, as we know that it does in the wake, it is regarded as a factor of secondary importance which is taken into account by the empirical quantity known as the "profile drag". But the resistance of a body, such as a circular cylinder,

which is not "stream-line" must be closely connected with the eddies which are shed from its rear surface, and the study of pulsating flow seems likely to assume increased importance in the future. Mr. Relf made a start in this direction in 1921, when he determined by aural methods, for a considerable range of speeds, the frequency of the eddies formed behind circular wires. Quite recently, in collaboration with Mr. Simmons, he has greatly extended the range of his investigation with the aid of a hot wire apparatus, and he has discovered an interesting correlation between eddy frequency and drag coefficient, which is shown in Fig. 11.

Hydrodynamic Stability

19. In my view, hot wire instruments of this kind (and Mr. Simmons has largely extended their range of usefulness during the last few months) offer the

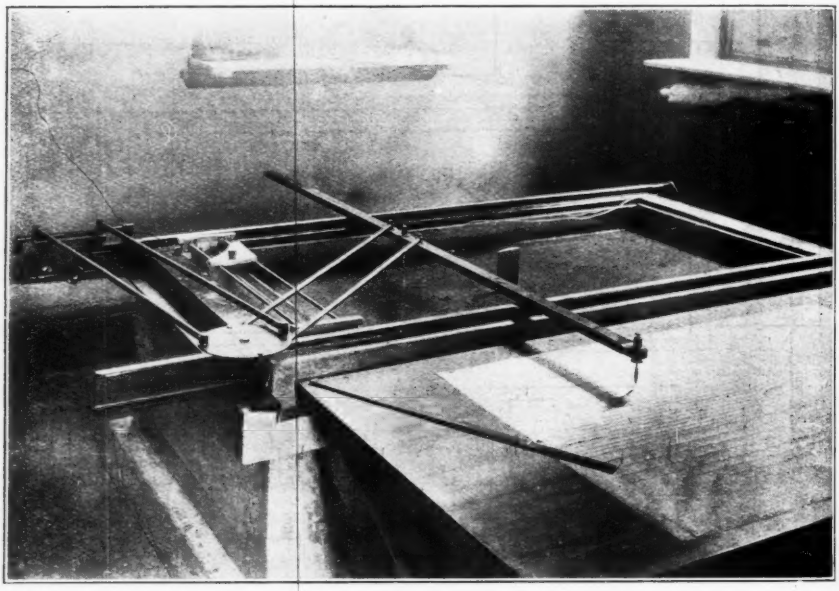


FIG. 10.

most hopeful means of experimental attack, if used in conjunction with selective resonators, on the difficult problems of the "boundary layer". Those who listened to Prof. Bairstow's recent lecture will realise that the conditions in this layer are the ultimate mystery of aerodynamics: somehow or other, in a film of air whose thickness is measured in thousandths of an inch, that circulation is generated which we have just seen to be the essential ingredient of "lift". From the experimental point of view, the conditions in the layer have every characteristic which one recognises as undesirable: there is an intense velocity gradient in a very narrow region, and the velocity must fall actually to zero at the surface; pitot tubes sufficiently microscopic for the work have been shown by Mrs. Glauert to be subject to appreciable scale effect, and Burgers in Holland has experienced difficulties with his hot wire instruments, arising from the cooling effect of the body tested. But cooling will not upset observations of *periodicity*,

and it seems possible that mathematical investigation of the stability of fluid motions will lead to theoretical estimates for the frequency of the eddies which are generated when instability sets in. The problems are so difficult that prophecy is dangerous, but I hazard this suggestion to-night because I am convinced that the conditions in the boundary layer form the problem of all others, in the fundamentals of fluid motion, which calls for attack in the next few years.

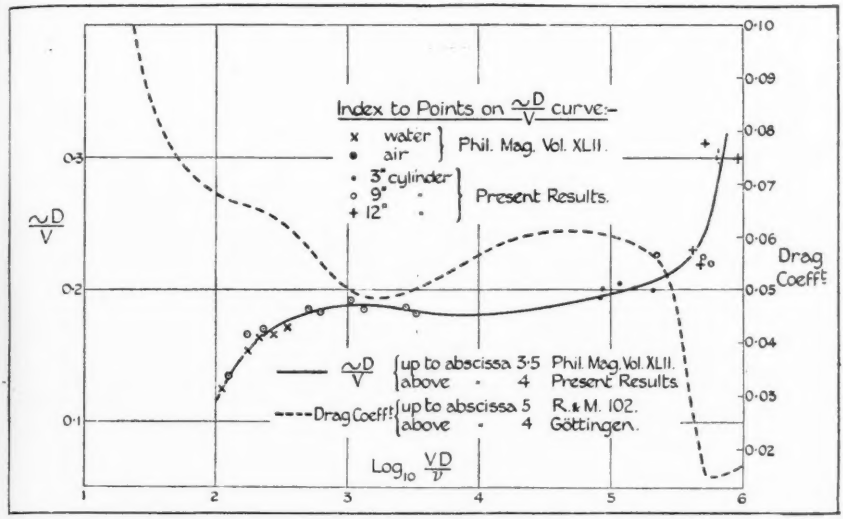


FIG. 11.

20. I have myself been trying for some six years now to lay the foundations of a theory of fluid instability, but I have not yet reached my goal, although I think that I have made some progress towards it. Perhaps I may be allowed to

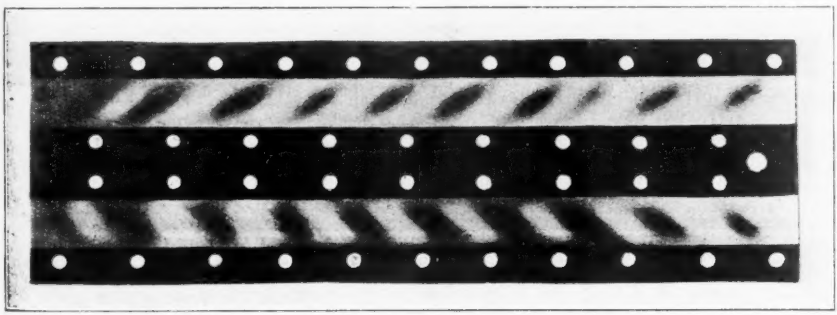


FIG. 12.

mention one of my results, on account of its rather interesting double application. Miss Skan and I recently determined the conditions under which a flat strip will be buckled by a shearing action in the way shown by Fig. 12. We were led by our results to suggest this " sheared strip test " as a criterion of

quality for the steel strip used in constructing metal aircraft, and exploratory work which has been done in the Engineering Department at Teddington indicates that the test will be satisfactory and useful; but it can also be shown that the theoretical figure for the shear required to collapse a flat strip (or rather, this figure multiplied by eight) fixes a limit to the rate of shear in a fluid contained between parallel walls, such that, unless this rate is exceeded, instability cannot possibly occur. An equivalent result had been obtained previously by Prof. Orr,

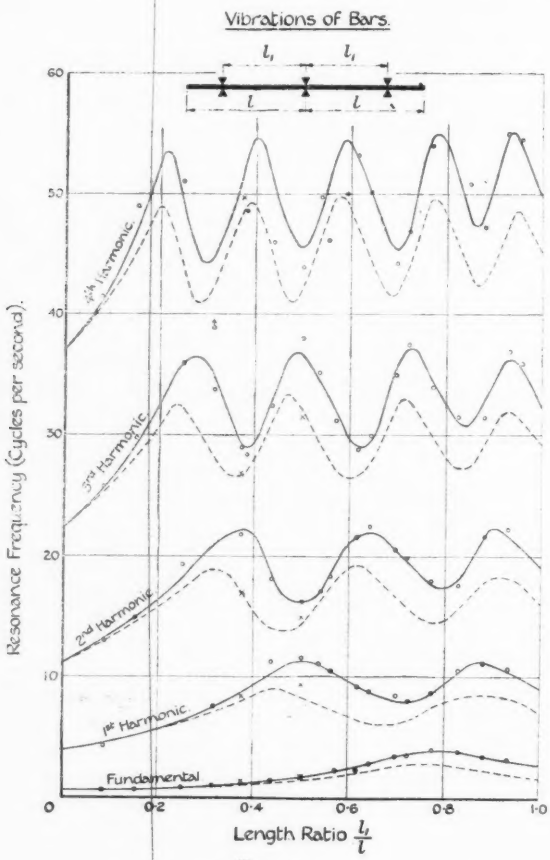


FIG. 13.

but it is always interesting to discover relations of this kind obtaining between different physical problems.

Elastic Vibrations

21. We have paid considerable attention in recent years to problems of elastic vibrations. My next slide (Fig. 13) shows some experimental results of Messrs. Relf and Cowley, and by means of the diagram at the top almost explains itself. The full lines relate to modes of vibration in which the two halves of the beam rise together, and the dotted lines to modes in which, while one-half is rising, the other is coming down; circles and crosses relate to corresponding

experimental results. What is important from the practical point of view is the crowding together, in some parts of the diagram, of several modes within a small range of frequency; for if the elastic system is regarded as representing the main spars of an aeroplane, this means that there must be an almost continuous tendency towards resonant vibrations as the engine speeds up through a certain range.

22. A problem of greater experimental difficulty has been recently attacked by Mr. Fage. As a first step towards the attainment of less noisy airscrews, it is necessary to trace the existing noises to their source. Much of the noise seems to be due to eddy formation, as in Mr. Relf's problem of the singing wire; but there may also be noises due to vibration of the blades, and it was thought that these would be more easily detected if their frequencies could be calculated in advance. The theory of lateral vibrations in a rotating blade was accordingly attacked in 1921, and methods of approximate calculation were worked out. Mr. Fage set out to test these conclusions by experiment.

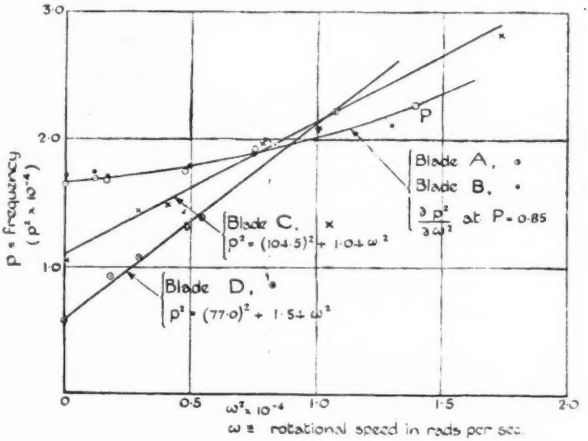


FIG. 14.

For convenience I shall begin with his results (Fig. 14). The theoretical formula for the frequency of vibration in terms of rotational speed gives a linear relation between the squares of these quantities, and you will see that in the case of two types of blades (denoted by the letters C and D) a relation of this kind is confirmed by experiment. Blades A and B gave less satisfactory results; but they were somewhat heavy blades with relatively weak roots, and it is doubtful to what extent the theoretical conditions were realised.

The method employed might be described as one of "cumulative resonance". The vibrations were to be detected by measuring the frequency of the sounds emitted, and the Tucker hot-wire microphones used for this purpose are, as is well known, very selective; they are, in effect, deaf to a noise of any frequency other than that for which they are tuned. But the *free* vibrations of the blades were not in general strong enough to be detected; they had to be intensified by "forcing", and of course all forced vibrations take the period of the forcing mechanism. Mr. Fage therefore determined the free periods of the blades by finding the conditions for maximum intensity, or resonance. He set up close to

his airscrew a wooden star (Fig. 15) with varying numbers of rays, each of which gave the blade an impulse as it passed; the period of the resulting vibration was of course the time occupied by a blade in passing from one ray to the next, and Mr. Fage tuned his resonator to conform with this at every speed of

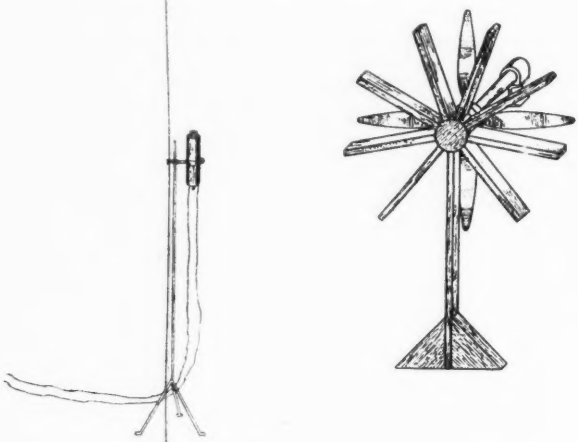


FIG. 15.

rotation which he employed. In this way he obtained finite readings at all speeds; but when the speed was such that a blade received impulses of frequency equal to its natural frequency of vibration the reading was largely increased, and thus the natural frequencies could be determined.

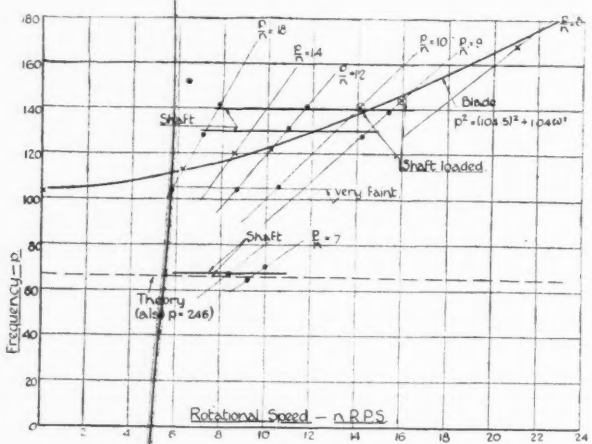
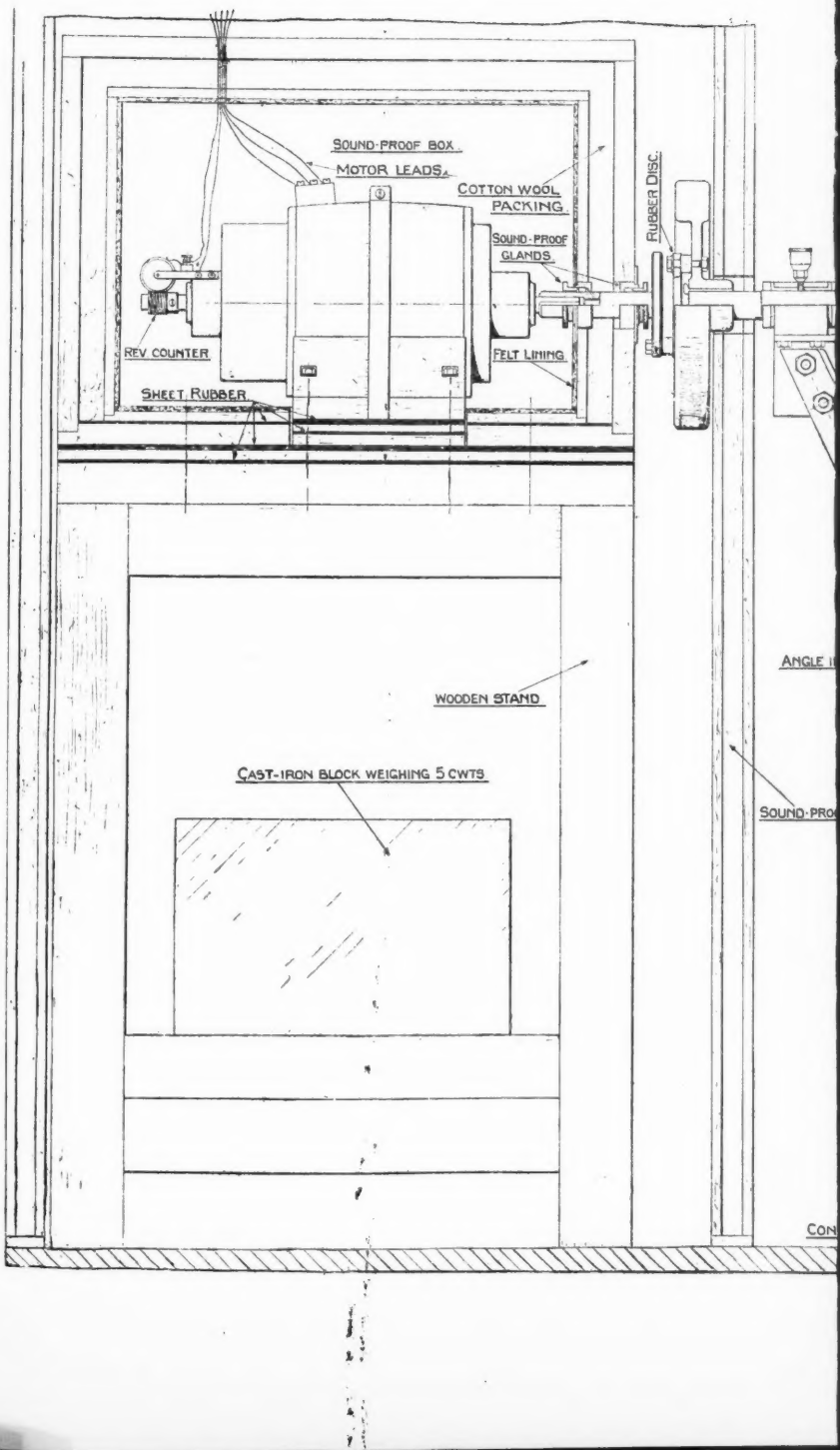


FIG. 16.

Fig. 16 illustrates the complexity of the problem, and the way in which theory was employed to disentangle the many observed resonances and to trace them to their source. Fig. 17 shows the general arrangement of the apparatus.


SOUND-PROOF BOX
MOTOR LEADS
COTTON WOOL PACKING
SOUND-PROOF GLANDS
RUBBER DISC
REV COUNTER
FELT LINING
SHEET RUBBER
WOODEN STAND
CAST-IRON BLOCK WEIGHING 5 CWTS

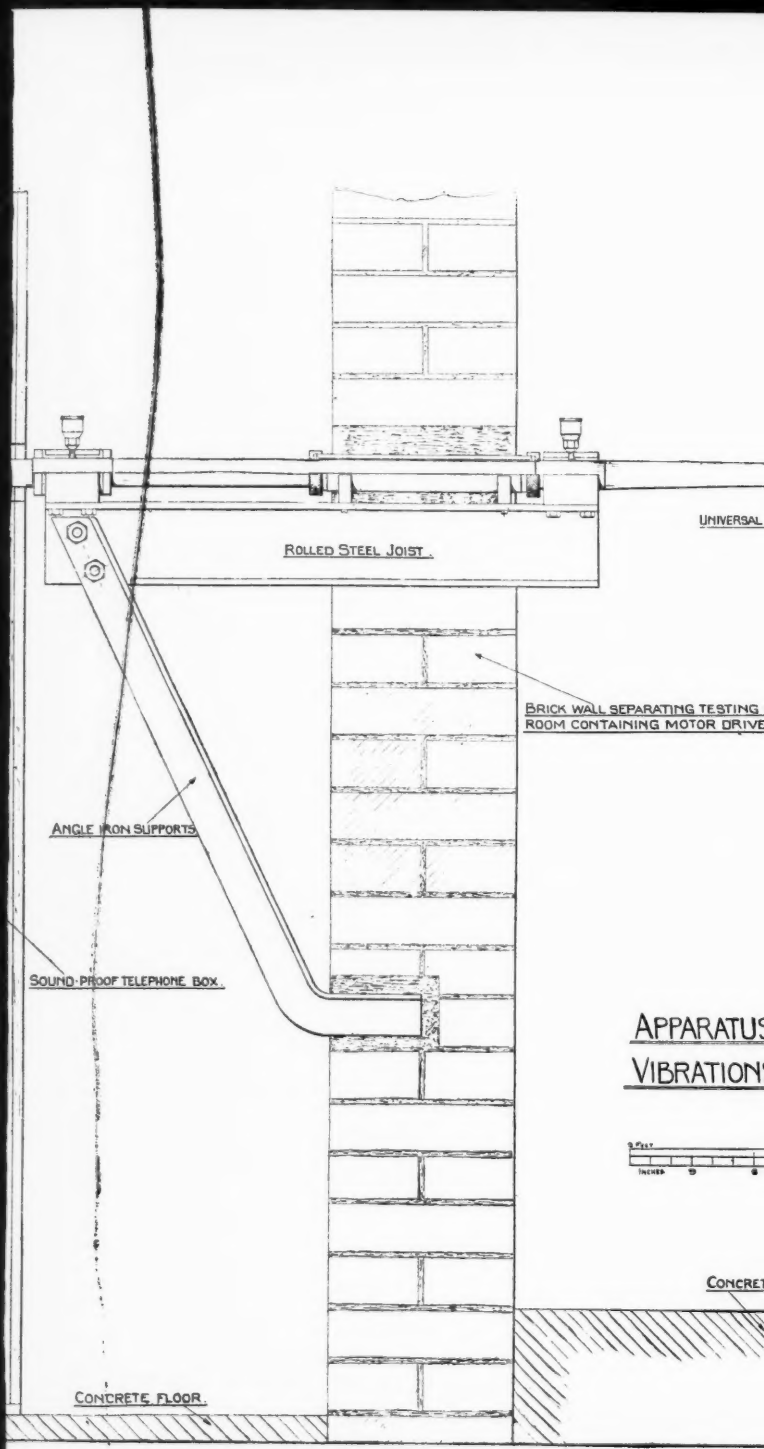
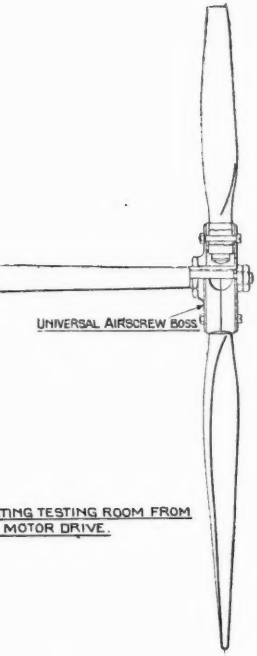
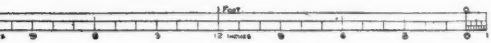

ROLLED STEEL JOIST.
UNIVERSAL
BRICK WALL SEPARATING TESTING
ROOM CONTAINING MOTOR DRIVE
ANGLE IRON SUPPORTS
SOUND-PROOF TELEPHONE BOX.
APPARATUS
VIBRATION
CONCRET
CONCRETE FLOOR.

FIG. 17.



TING TESTING ROOM FROM
MOTOR DRIVE.

PPARATUS FOR EXPERIMENTS ON
BRATIONS OF AIRSCREWS.



SCALE.

Conclusion

Here I end my survey of our recent work, and I might add that I have asked those colleagues whose work I have described to attend this meeting, in order that they may be able to reply to questions of detail. I regret that in the time available I have had to make a purely arbitrary selection out of many researches which I should like to have described to you.

Since this is the last occasion on which I shall be speaking for the Aerodynamics Department of the N.P.L., I may perhaps be forgiven if I close with an expression of personal conviction. I have tried to illustrate the development, in the last few years, of a more theoretical attitude to the problems of aeronautics; I believe (although I recognise the strength of the case which can be opposed to me) that it is more fruitful, in the long run, than the attitude which is generally termed " practical "; and I want now to urge that *if* I am right (and I believe that this Society as a body holds views which in principle, at all events, agree with my own), then we must recognise the need for ensuring the utmost freedom which is possible for the individual research worker.

In constituting " theoretical " and " practical " attitudes to research I mean this :—At any given time (next month's meeting of the Aeronautical Research Committee, for example) there are a number of questions which need an answer for purposes of immediate practical importance; they can be settled by tests *" ad hoc "* in the wind-tunnel or elsewhere, and an expert committee can control these tests by advice, co-ordination and decisions in regard to priority: I should say that researches so initiated are undertaken from the " practical " attitude. But in another category are researches which develop out of a new idea—a " brain wave "—in the mind of some individual thinker; the vortex theory of aerofoils will serve my purpose as a leading example, and I have tried to indicate others which have originated at the N.P.L. in the way I have described. Now in my view there is only one way of prosecuting these researches: you must let the ideas " take charge ". They will lead you in unforeseen directions; they may even take you out of your subject into another which you would have said had no relation to it (as exemplified by the analogue I have mentioned between the sheared fluid and the sheared elastic strip); but wherever they lead you must follow. This, it will be said, is a truism,—the experience of everyone who is engaged on original research. Yes, but if so, and if we believe in research of this nature, we must provide the conditions in which it can grow, and foremost among these, I would urge, is freedom and leisure for the individual research worker.

At the National Physical Laboratory (and of course the same is true of the Royal Aircraft Establishment, but I am limited by my title) there is magnificent and costly equipment, and there is also, I have tried to show, a body of men who are capable, given the opportunity, of developing self-initiated investigations along original lines; we owe a debt of gratitude to this Society, to the A.R.C., and not least to the present Director of Scientific Research, for the freedom which they have enjoyed in recent years. I trust that the same freedom will be given them in the future. If they are required to concentrate on an intensive programme of " practical " investigation they will do so,—and I hope no one will understand me to suggest that their wide experience of wind-tunnel technique will not thereby be employed to the full. But if every hour of their day is earmarked for work on definite programmes which are revised at short intervals by committees, then we are lessening the prospect of new and original ideas. Have we faith enough in the ultimate emergence of a real *science* of aeronautics to be willing to pay a price for the chance of a new idea? Personally I have no hesitation in saying that the Vortex Theory of Aerofoils would have been cheap at the cost of a wind-tunnel left idle for a year; and although that is not the form in which the

choice will be presented, I submit that there is a choice of policy always confronting the men who direct aeronautical research,—a choice between equipment and personnel, between data and ideas.

Discussion

Dr. Piercy said he had been very interested in the lecturer's remarks on the most desirable manner in which research should be conducted. An authority had once expressed an opinion to the effect that an experiment without a theory was like a ship without a rudder. Another had suggested that the proper way in which to approach an experimental research was by obliterating all previous knowledge of the subject. He thought a middle course, first set in a direction suggested by a promising theory, but with sufficient freedom to take full advantage of inspiration originating in the work, should lead to the most useful results.

The lecturer had given them an admirable account of a great deal of very ingenious and successful research. The ground covered was so extensive that detailed discussion was impossible. He would like, however, further information regarding the observed anomaly of L_0 failing to change sign beyond the critical angle. Was this unexplainable, by the usual methods, from the form of the lift curve; had it been traced to a looping of the lift curve near the critical angle; or had it to be left as due to the general instability of the lift in this region?

Major Southwell said that Dr. Piercy's question was very difficult to answer, since one did not know what the lift curve was; it was so indefinite about the critical angle. One did not even know whether there were two curves or one twisted back on itself, and hence it was not possible to say either that the results agreed with what was predicted or that they did not. When the wing which was being rotated was also being alternately stalled and unstalled it was difficult to make comparisons with theory, whether "Prandtlised" or not.

Major H. E. Wimperis said he had listened with a great deal of pleasure to this able lecture, and it was of particular interest to have such a summing up of the work done at the Aerodynamics Department in recent years. This was a specially important occasion, as it coincided with what was probably the last occasion on which the lecturer would speak to them in his capacity of Head of the Aerodynamics Department.

Before saying anything further he would like to ask the lecturer about a slide illustrating an experiment in a tank. Was that experiment intended to represent motion in a viscid fluid or an inviscid fluid?

A great deal of the work at the National Physical Laboratory and the Royal Aircraft Establishment related to the question of stability, and that meant not merely finding a means of making a machine more stable in normal flight, but elucidating the cause of certain very curious phenomena—phenomena with which they were confronted in certain modern machines which were found to have peculiar, and sometimes very troublesome, properties in spin. With the increasing speeds of modern machines these phenomena became of increasing importance. It was very difficult to make experiments in the air; indeed, it was often too dangerous. It was therefore important that the wind tunnel should help them as far as it could, and they needed very accurate measurements. Until these phenomena were better understood, and the scale effect brought to a useful degree of completion, it was impossible to know when designing a machine whether it would be reliable or not. Now it was a long business to get to the bottom of questions of that nature, and it meant taking up the time of the wind tunnels. The wind tunnels were at the disposal of the Director of the National Physical Laboratory, and therefore they at the Air Ministry were very grateful to him for allowing the use of the wind tunnels to be available for work of that

kind. He thought it would be a very serious state of things if such investigations were delayed by other work, since they were so strikingly important.

Major SOUTHWELL said that he and the staff of the Aerodynamics Department were very much indebted to the Director of Scientific Research for his expression of appreciation of the work done at Teddington. They were very happy in the conditions under which they had been able to work, and he had tried to show in his paper that that liberty had not been misapplied.

With regard to Major Wimperis's specific question, the electrical tank was there to do what the mathematician could not or would not do, because it meant such an enormous amount of calculation. The problem to which it gave an answer was that of tracing streamlines for an inviscid fluid. One needed to know that pattern in order to determine how far actual streamlines conformed to the flow of an ideal inviscid fluid; this was the starting point of the Prandtl theory, from which so much was deduced in regard to the effect of aspect ratio, interference of tunnel walls, etc.

Regarding the strange behaviour of certain machines, it was unfortunate that measurements in themselves never enabled one to find a remedy; it was only in the light of a long series of experiments that one came gradually to the conclusion that some characteristic made for or against satisfactory behaviour. At the same time, there was nothing one could do about it for the present except to go on measuring and examining and trying to develop a fundamental theory. Some time in the future one might hope to be able to do something better than merely generalise from data, and this was one of his main reasons for urging the importance of fundamental research: one could not be satisfied to leave aerodynamic theory in a condition which, to use the words of Prof. Rutherford, resembled "a branch of descriptive botany."

Major A. R. LOW said those who expected lively criticism from him would be disappointed, for on the valedictory appearance of Major Southwell in connection with the National Physical Laboratory, he felt that minor points of controversy might be left aside, and on the general question he had nothing to express but unqualified appreciation of the lecturer and of the work described in his paper.

The equipment and technique of measurement at the N.P.L. had always set a high standard.

In aeroplane stability, as they all knew, Bryan's mathematical theory had been brought down to effective practice by Major Southwell's predecessor, Professor Bairstow, and his collaborators, who by numerous ingenious experimental devices, accompanied by appropriate mathematical developments, had obtained all the required coefficients.

He had followed that work as far as small displacements within the flying range, but when asked by Service men, as he had frequently been asked in 1916-1918, about the cause and possible cure of spin, he had only been able to reply that the problem should be referred to Professor Bairstow, and that had proved to be the right answer. He would select the elucidation of spin as the most impressive achievement of British aeronautical science. Without the concurrent development of mathematical theory and experimental determinations they would have been entirely in the dark on these vital problems.

In that respect we had certainly led the world, and it might fairly be set off against our missing the development of Lanchester's theory, which had now come back from Germany. He would go further than the lecturer, and state that early possession of the Lanchester-Prandtl theory would have economised the work of many wind tunnels for many years.

The root of the matter was that in Germany there was full appreciation of the mutual benefits obtained by co-ordinating abstract mathematical and scientific

development with technical processes. They offered positions giving the necessary time and means for such work to a far larger number of individuals, and granting that the best men in the two countries were equally good, their more numerous workers and their more systematic survey of the work done by other nations gave them a proportional advantage in following up difficult and abstract lines of mathematical research in conjunction with more concrete experimental methods.

Here they had a handful of able men working on hydrodynamical applications, there five times as many workers of comparable merit. Thus they had fallen ten or twenty years behind, though he agreed that it need not take nearly so long to bring themselves up to date again.

He himself had dropped the study of formal hydrodynamics for many years on account of the unreality of many of its leading results. Since 1921 he had taken up the subject again in connection with the Lanchester-Prandtl theory and had given to it the whole of his spare time and such energy as was left over from his routine duties. He began to feel himself in a position to appreciate the severe mathematical difficulties referred to by the lecturer, and to hope that he might take part in further progress. He believed that the aeronautical industry in this country would support due expenditure allowing of real research (as distinct from the development of design, which was properly the business of aeroplane designers) by scientific workers as numerous as in Germany and as free to follow up purely scientific lines of inquiry, in the confident belief that it would repay them manifold.

Major SOUTHWELL said that he wanted to make one point in connection with the Prandtl theory: he did not think we were so far behind the Germans, in the theoretical work associated with that theory, that we could have no hope of being "in at the death", because the theory, in so far as it involved the notion of circulation of irrotational type in an infinite field, was necessarily not exact. The really interesting part of Prandtl's work was the work which he had been doing subsequently in his study of the "boundary layer", because that work might ultimately explain why with assumptions which could not be correct one could make such amazingly true predictions in regard to problems such as the influence of the tunnel walls upon the results of model tests. That the assumptions of the standard Prandtl theory did in fact depart from the truth was perhaps most clearly shown by the results for the Rotor ship: if the circulation which gives rise to the lift of a rotating cylinder were in fact of the nature of the irrotational circulation of an inviscid fluid, there would be no limit to the lift which could be obtained at sufficiently high speeds of rotation; yet experiment indicated a limit which is reached comparatively soon. The last word had not yet been spoken, and there was still work to be done in which this country could take its share.

Major LOW, after a reference to a note by himself on "Rotor Ships" appearing elsewhere in the Journal, concluded by saying that it was a grievous loss to the N.P.L. that a man of Major Southwell's calibre was leaving, but he was not lost to aeronautics, and would continue his good work in Cambridge University, where he would have a unique opportunity of directing attention from the study of hydrodynamics too much as a mathematical abstraction in certain aspects, to its development as a truly physical theory of the phenomena of flight.

Commander J. C. HUNSAKER expressed his appreciation of the work of the National Physical Laboratory and the great help it had been to aeronautical engineers and the science of aeronautical engineering. He thought that the alleged German superiority in research was largely a legend. He could not agree with Major Low. In successful research in any country some one man with initiative did it almost all, helped by a few students, who carried his work farther. Genius could not be organised. There should be some men free to do

pure research work and others organised to do the hack work required by the industry, who were inclined to think it quicker and cheaper to make a machine and try it than to wait for the research people to solve their problems. Two types of research staff were clearly needed—one with complete freedom to investigate fundamentals and one controlled rigidly by a schedule of priorities to test applications and to obtain specific data for use in practical design work.

Mr. F. HANDLEY PAGE said that he had not intended to take part in the discussion, but as he understood that it was the last occasion on which Major Southwell would be with them in his present capacity he wished to join his appreciation with the others already expressed. He did not agree that the German scientists were superior to the English ones. The Germans did not produce the real foundations of aeronautical science on which everything was founded. Prandtl's theory originated with Lanchester. Really valuable work was done by the old Aeronautical Committee. The American National Advisory Committee was to a certain extent founded on the model of the institutions of this old country. When one really looked into the problem of what was required in research work one found that the first thing was genius, and particularly the genius that was able to select genius, and a storehouse of genius on which to draw was necessary. In England they had been extraordinarily fortunate in that respect as regarded aeronautics. The industry owed a great deal of gratitude to the scientists for enabling them to turn out such machines as they had. They had a real admiration of the ingenious methods adopted at the N.P.L. to produce such research work.

Mr. W. O. MANNING was of opinion that the aircraft industry was ahead of all others in the application of science. One had only to talk to people employed in other branches of engineering to find how much less they knew of the science of their subject than aircraft workers did. The stresses in the case of an aeroplane were undoubtedly got out with greater accuracy than in any other branch of engineering. That these things are so is largely owing to the work done at the National Physical Laboratory. He certainly agreed that those men who showed a special aptitude for pure research should be encouraged, for only in that way could this country keep ahead. He hoped Major Southwell's successor would be as successful as he had been.

A hearty vote of thanks was passed to the lecturer.

Major SOUTHWELL said that the appreciation of their work expressed in the discussion would be very encouraging to the Aerodynamics Staff of the National Physical Laboratory, who would feel that they had a backing in their desire to attack the fundamental problems confronting them. None of them, he believed, laid claim to genius, and he had tried to make it clear that they had no feeling that their capacity for original work had been hampered. They had been very happy in having been allowed freedom in the past to elaborate such ideas as came to them.

PROCEEDINGS

THIRD MEETING, SECOND HALF, 60TH SESSION.

A joint meeting of the Institution of Automobile Engineers and the Royal Aeronautical Society took place at the Royal Society of Arts, Adelphi, on Thursday, February 19th, 1925, Mr. Kerr Thomas, President-elect of the Institution of Automobile Engineers, presiding.

The CHAIRMAN, in introducing Colonel Fell, apologised for the unavoidable absence of Colonel Tizard, which accounted for his presence there. As a Member of Council of the Institution of Automobile Engineers, it gave him very great pleasure to be there, and he was particularly glad that an occasion had been made for a joint meeting, because of the great interest the subject had in common for both bodies.

LIGHT AEROPLANE ENGINE DEVELOPMENT

BY LIEUTENANT-COLONEL L. F. R. FELL, D.S.O., O.B.E.

It has frequently been stated and written that in order to popularise light aircraft the first essential is the production of a reliable engine capable of being easily maintained and having a long life, at the same time selling at a low figure. In the first part of this lecture it is desired to point out the difficulties in the way of realising this ideal before remarking on the claims of the various types for adoption.

Difficulties in the Way of the Production of Light Aircraft Engines

In the first place the public, and even aircraft designers, have been misled as to the type of engine that is required by statements made in the non-technical and semi-technical Press to the effect that it is possible to fly an aeroplane satisfactorily with a motor cycle engine. At this stage it is desired to state quite definitely that this is impossible, as figures, which will be given later, clearly indicate.

The method of rating on capacity, instead of on a h.p. basis—the normal manner for aircraft engines—has also caused considerable misapprehension and called forth the statement that a complete motor car with an engine of 1100 c.c. capacity can be purchased at the same price as a light aircraft engine of similar capacity. It is the case with most prime movers, and especially so with internal combustion engines, that it is the normal h.p. that an engine can maintain indefinitely which has to be paid for, and when the light aircraft engine as we know it to-day is examined in this light, it will be found that it is not a particularly expensive prime mover. A car, or a motor cycle, driven as hard as British roads will allow, does not exceed an average h.p. greater than about one h.p. per 100 c.c. Very few road vehicles are put to as severe duty as is indicated by this figure, and the £175 variety does not withstand such treatment for very long. As is shown in the following Table I., the average h.p. taken from the various engines used in the Lympne competition was 34, or about three times the normal for a motor car engine of similar size. In fact, the output from these competition engines compared very favourably with the best Brooklands efforts of engines of the same capacity.

TABLE I.

ENGINE.	R.P.M.	B.H.P.	AIRCRAFT.	REMARKS.
Cherub ...	3200	32.6	Beardmore	Direct drive (cruising)
,, ...	3600	34.0	,,	High speed trial
,, ...	4000	36.0	Avis ...	Geared
,, ...	3600	34.0	,, ...	Direct drive
,, ...	4010	36.2	Pixie ...	Geared (two parts)
,, ...	3000	31.0	Brownie ...	Direct drive (cruising)
,, ...	3200	32.6	,, ...	,, ,, (high speed)
,, ...	3000	31.0	Cranwell...	,, ,, (cruising)
,, ...	3200	32.6	,, ...	,, ,, (high speed)
Blackburne ...	3600	36.0	Sparrow ...	,, ,, (cruising)
,, ...	3800	38.0	,, ...	,, ,, (high speed)
,, ...	3800	38.0	Pixie ...	,, ,, (cruising)
,, ...	4000	40.0	,, ...	,, ,, (high speed)
Anzani ...	3000	31.0	Hawker ...	,, ,, (cruising)
,, ...	3600	35.0	,, ...	,, ,, (high speed)
A.B.C. ...	3200	32.0	,, ...	,, ,, (cruising)
,, ...	3600	34.5	,, ...	,, ,, (high speed)

It will be readily understood, therefore, that the engine for light aircraft must be a machine far superior to anything that runs on the open road and, in consequence, more costly in direct proportion to its power output. The requirements of the satisfactory light aircraft engine are precisely the same as for large aircraft types, viz. :—

(A) Reliability.
(B) Light weight in working order.

(A) can only be obtained as the result of careful design and laborious and expensive testing on the bench. (B) is actually more difficult to attain in a light aircraft engine than it is in one of 500 h.p. for the following reason. Though it is true that a slightly higher h.p. per unit of cylinder volume is permissible with the small engine, certain parts—such as cylinders—have to be made thicker and consequently heavier than is dictated by stressing, in order to obtain the necessary rigidity. In an engine of about 500 h.p. this condition does not arise. The use of a higher h.p. per unit of cylinder volume is, of course, strictly limited by propeller speed unless gearing is to be introduced. The latter is undesirable as, besides putting up the cost of the engine, it has been proved in engines of all sizes that the reliability is materially decreased and vibration is introduced unless the engine is multi-cylindered or fitted with a flywheel, with consequent increase of weight.

In order to indicate the relatively high duty obtained from the engines at the Lympne competition, Table II. has been prepared. From this table it will be seen that, in one case, the B.H.P. per cubic inch of cylinder volume was nearly 1.8 times that taken from the Napier "Lion," Series II., engines at maximum permissible r.p.m., *i.e.*, the speed allowable for a few minutes' burst only.

TABLE II.

	Type of Engine.	B.H.P. per cubic inch cyl. vol.	B.H.P. per sq. in. piston area.	R.P.M.	Total B.H.P. developed.
A	Lion II. ...	.343	1.76	2200	503
B	Condor III....	.33	2.475	2200	705
C	Cherub ...	.33	1.23	2200	22.5
D	Blackburne ...	.357	1.36	2200	24
E	Cherub ...	.49	1.89	3200	32.6
F	,, ...	.54	2.0	4010	36.2
G	Blackburne ...	.557	2.135	3600	37.5
H	,, ...	.61	2.333	4000	41.0

In passing it is thought that it is of interest to point out that during the competition certain British engines were running at powers fully 50 per cent. in excess of their normal maker's rating. That unreliability was experienced is not therefore surprising. It must be remembered that just because an aeroplane engine is small it is no less likely to break down when run beyond its rating than is the engine of 400 h.p. or 500 h.p.

As has already been pointed out, a good power/weight ratio is more difficult of attainment on the light aircraft engine than it is on the larger engines, and even when run at the high ratings given in Table II., none of the engines can be considered as having a good power/weight ratio when compared with larger engines. Not one approached 2 lbs. per B.H.P., and were mostly around 3 lbs. per B.H.P. and over. In this connection the light aircraft engine is at a great disadvantage owing to the large proportion of the total weight absorbed by what may be termed auxiliaries, such as ignition gear, carburettors and oil pumps. Very little can be done to reduce the weights of these parts, and it has been roughly calculated that the percentage of the total weight taken up by auxiliaries is quite three times that of the engine of 500 h.p.

As regards the carburettor, altitude control is found to be a necessity. In order to get good results from the engine the very finest workmanship is required on this control. Even on large engines, unless the highest degree of refinement in manufacture is put into the construction of the carburettor, and altitude control in particular, trouble is experienced. The light aircraft engine will obviously be more sensitive still to any air leakage.

As regards ignition, dual ignition is obviously as essential to reliability on the light aircraft engine as on all other aircraft engines. Merely to provide a magneto firing two sparking plugs is insufficient, for it must be borne in mind that the cylinders are working at as high a M.E.P. as is usually adopted on aircraft engines, and the sparking plugs and magneto are therefore just as liable to failure.

In order to ensure correct lubrication at high speeds without over oiling and consequent high oil consumption, a positive dry base oil system is indispensable. With roller bearing big-ends, this oiling system is a particularly difficult problem, for contrary to the time-honoured statement of the ball and roller bearing manufacturers, the roller bearing big-end run at the speeds and loads called for on the light aircraft engine requires considerably more oil ' than is required to prevent rust." To supply this oil without over oiling the cylinders calls for very great care in design and perfection of workmanship.

The design and manufacture of connecting rod big-ends for a radial engine is probably the most serious problem to be solved in this type of engine, and is the limiting factor in its development. Owing to the high speeds and outputs for light aircraft engines, the solution of this difficulty is no less serious in their case.

Finally, the light aircraft engine is a high efficiency engine, comparing so far as the motor vehicles are concerned with a track racing car engine only. The most thorough design, the finest materials, and the highest class of workmanship only can therefore be employed in its manufacture if success is to be achieved. It will readily be admitted that these requirements are incompatible with cheapness of production.

Suggestions and Remarks Regarding the Choice of a Type and Design Generally.

(a) Method of Rating.

As already indicated, the writer is of the opinion that the method of rating by capacity is definitely undesirable. The choice of the cylinder capacity should be left to the designer and should only be settled by him after selection of the

type of engine which he wishes to build. As is the case with all other aircraft engines, for any given h.p. the multi-cylinder high speed engine will have a smaller capacity than an engine employing a few larger cylinders, both designs being of similar weight and giving equally good reliability. It is safe to say that had the 1100 c.c. rating not been a necessary condition for the engines to fulfil in the Lympne competition, two-cylinder engines would have been produced with larger bore cylinders running at lower speeds, and having very much greater reliability at a negligible increase of weight. The adoption of the capacity rating in general and of the 1100 c.c. in particular was fully justified at the time. It was considered that motor car and motor cycle engine manufacturers were used to this method of rating and, in most cases, had designed their engines to fall into certain clearly defined categories. It was hoped, therefore, that more entries would be encouraged from the motor trade if it was made possible for them to introduce their standard models for light aircraft purposes. Having established the capacity basis, and in order to make the competition of the greatest value to aeronautics, it was obviously desirable that the smallest engine that could possibly do the work should be specified. This was assumed to be the 1100 c.c. As the motor manufacturers have not been able to see their way to enter their standard models, in view of the heavy duty called for, the reason for retaining a method of rating foreign to aircraft practice no longer exists.

In the writer's opinion, the normal ground level h.p. and propeller speed capable of fulfilling the duties of the various classes of light aeroplanes should first be determined and laid down, and the engine builder thereafter be given a free hand to produce an engine of the best power/weight ratio he can at the h.p.'s and speeds given with no other restriction than that his engine must be capable of passing the Air Ministry's standard test of reliability as given in Part "B" of Air Publication 840.

(*b*) *Air versus Water-Cooling.*

It is somewhat astonishing, in view of the competition between water and air-cooling on large aero engines, that the protagonists of water-cooling should have left the field clear for air-cooling.

It has already been proved that the air-cooled engine is not necessarily lighter or cheaper than the water-cooled and, on the other hand, that the water-cooled engine can be produced to give considerably less head resistance than the air-cooled. This latter feature is clearly indicated in Fig. 1, which is a comparison

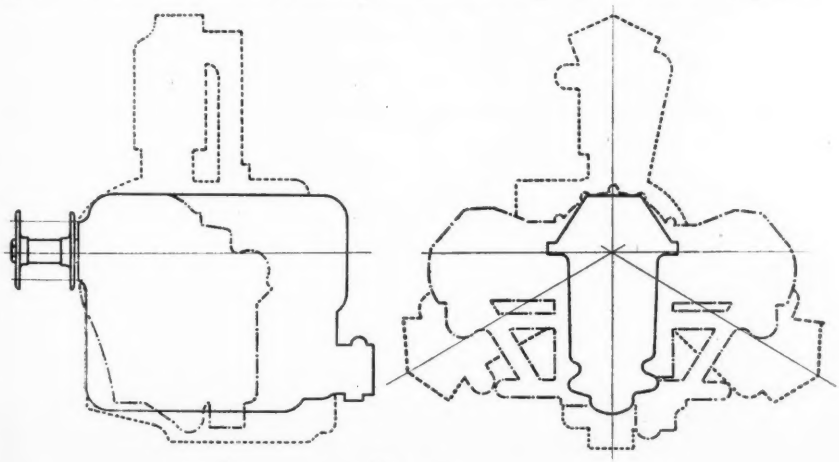


FIG. 1.

between a flat twin air-cooled, a three-cylinder radial air-cooled, and a four-cylinder in line inverted water-cooled with a special form of radiator, all the engines being of equal capacity. These types will be referred to in detail later.

As regards reliability, from the motor car manufacturers' point of view, there can be no doubt that the water-cooled engine is supposed to be superior to the air-cooled.

Owing to the cylinder block construction of the water-cooled, great rigidity and, consequently, less vibration is obtained from the water-cooled engine. As will be indicated later, it is a simpler matter to make a multi-cylindered water-cooled engine than it is to make an air-cooled multi-cylindered engine. Higher speeds of rotation are, therefore, more easily obtained with the former than with the latter. This, of course, allows of an engine of smaller capacity for a given duty than can be obtained from an air-cooled engine of normal type.

In the writer's opinion, these advantages are quite sufficiently great to warrant serious consideration of the water-cooled type for light aircraft. Further, there are quite a large number of water-cooled engines in use on the track capable of performances equal to the requirements for light aircraft; these engines would only require developing in a lighter form, probably by the substitution of light alloys for cast iron, in order to make them suitable.

(*c*) *Supercharging.*

It is considered that the application of a supercharger to the light aircraft engine is a problem well worthy of investigation by designers, in order to make the maximum use of the cylinder capacity without resorting to excessive speeds. By supercharging, the writer only wishes to suggest the use of this device as it is used on the track, that is to say, for the purpose of obtaining an output greater than normal from a given capacity and not as it is known in aircraft, *i.e.*, with a view to maintaining ground level power at altitude.

As an instance of what has already been done on motor cars in this connection, the following figures relating to the Sunbeam 1500 c.c. racing cars are of interest:—

Bore and stroke	67 mm. × 105.5 mm.				
Volume	1487 c.c.				
1922 engine developed	50	B.H.P.	at	4,000	r.p.m.
1923 ,, ,,	60	,,	,,	,,	
1924 ,, ,,	72	,,	,,	4,500	,,

	Engines.		
	1922.	1923.	1924.
B.H.P. per 100 c.c. ...	3.33	4.0	4.66

For purposes of comparison with light aircraft engines in use at Lympne these figures have been converted to a 1100 c.c. basis; they then read:—

1922 engine ...	36.63	B.H.P.	at	4,000	r.p.m.
1923 ,, ...	44	,,	,,	,,	
1924 ,, ...	51.26	,,		4,500	,,

The 1922-3 engines were fitted with four valves per cylinder, whereas the 1924 type was fitted with two valves per cylinder, but was supercharged by means of a Root's blower driven direct off the crankshaft. In 1922 the fuel used was 50 per cent. benzol and 50 per cent. aviation spirit, whereas, in order to get the increased performance in 1923, alcohol was necessary. Owing to the use of the supercharger, however, it was possible to return in 1924 to the aviation spirit only in spite of increased h.p.

It is thought that only the geared type of supercharger of any of the well-known types—such as the centrifugal, eccentric vane or Root's blower—should

be considered for light aircraft engines, as the additional complication, expense and weight of the exhaust driven turbo-compressor type would not be justified in view of the comparatively light duty required from the supercharger.

(d) Relative Merits of Different Types.

"V" Twin.

In the writer's opinion, this type of engine has no particular merits for aircraft propulsion. Should it be used, however, it is essential that it should be constructed as what is known in the Air Ministry as an inverted engine, that is to say, with the cylinders below the crankcase in order to get the exhaust away conveniently and also that it may not interfere with the pilot's view. The ordinary wet sump motor cycle engine is obviously unsuitable for this reason, even were these engines up to the duty. The main reason for the popularity of the "V" twin is that it can be conveniently accommodated in the normal form of diamond frame almost universally adopted in motor cycle practice. The balance of engines of this type is poor and the firing interval is, of course, uneven, both of which facts militate against its successful use in light aircraft owing to the difficulties in securing satisfactory mounting and eliminating harmful vibration on the machine structure, the instruments and the pilot himself.

180° Flat Twin.

From the point of view of simplicity of design and production this engine is perhaps the best type. As in the case of the "V" twin, unless a very strong mounting is used the torque fluctuation of a two-cylinder engine is apt to result in damage to the machine structure. Also, it seems that there is a limit to the cylinder capacity which can be used in an engine of this design owing to difficulties experienced in balancing when the bore is more than 90 mm. Attempts so far with engines beyond this size have not been satisfactory. The carburation of this type of engine is also a source of trouble and, in many cases, it has been necessary, in order to overcome this, to fit a separate carburettor to each cylinder. Owing to the comparatively large dimensions of the cylinders, this type of engine is not particularly suitable for running at high speeds and cannot, therefore, make the best use of its capacity. The fitting of a supercharger should, however, enable a reasonably well-balanced 1100 c.c. engine to be produced giving about 38/40 h.p. at a speed of under 3,000 r.p.m. From the point of view of air cooling this design presents the least difficulties of any type in that, in addition to the fact that the cylinder head is well exposed to the slipstream, it is also easy to provide in the machine design for a clear exit for the air at the back of the cylinder. As regards lightness, it is considered that within certain limits of power, and provided the designer is given a free hand regarding the bore and stroke, it would be possible to produce an engine of this type with a power/weight ratio comparing favourably with any other.

180° Flat Four.

An experimental engine has been built in France having four cylinders in two blocks of two at 180°. It is understood that the results obtained have been quite satisfactory and that the engine is singularly free from vibration. No trouble appears to have been experienced with the cooling of the rear cylinder, though it would seem that cooling, and consequently, the output of the front and rear cylinders would most certainly be different. This form of engine has the following advantages over the ordinary flat twin :—

1. The overall dimensions are reduced.
2. The frontal area is reduced.
3. Balance is improved.
4. Torque recoil on the machine structure is less.

5. If it is admitted that there is a limit to the capacity to which the flat twin can be built, it is obvious that the flat four can be designed up to double the capacity.

It is considered that this design is worthy of consideration by British designers.

Three-Cylinder Radial.

As is shown in Fig. 1, the frontal area of this type is very high compared with other types, and there does not seem to be any prospect of reducing this, as a larger crankcase than is required for the 180° twin is necessary in order to clear the triple connecting rod arrangement, and the resistance of the third cylinder would be only slightly less than that of one of the cylinders on the flat twin of equal capacity. On the score of weight this type comes out considerably heavier than the flat twin partly due to the fact that cylinder weight per B.H.P. increases as the size gets smaller, as already pointed out. There are certain difficulties in the construction of this type, chief of which is the big-end bearing. The balance of the three-cylinder radial is, however, superior to the flat twin, and, of course, the torque recoil on the machine is less, thereby rendering the mounting a simpler proposition. A neat induction system for a three-cylinder radial is not easy to arrange. It is obvious that the construction of the crankcase and connecting rods will be far more expensive than in the case of the flat twin. From the point of view of cooling it is not so easy to arrange satisfactory egress of the air behind the vertical cylinder, and the exhaust outlet from this cylinder is in an inconvenient position from the pilot's point of view.

Five-Cylinder Radial.

Owing to the method of rating on capacity, this type of engine was, practically speaking, ruled out of the Lympne Competition owing to the very small sizes of cylinders which it would have been necessary to employ to conform with the regulations. It is considered, however, that this type has much to recommend it. In a radial engine, in order to get light weight per h.p., it is necessary to fit as many cylinders around the crankcase as possible. The crankcase to accommodate five cylinders is no larger in diameter or heavier than that required to accommodate three. The connecting-rod big-end difficulties are not seriously increased, and some form of induction manifold can be introduced conveniently. From the point of view of head resistance and power/weight ratio, this engine would be superior to both the flat twin and the three-cylinder radial. Larger capacity and, consequently, lower speed, is, therefore, permissible. In the writer's opinion, though somewhat expensive, this is a very promising type for light aircraft.

Swashplate Engine.

An engine in which the axes of the cylinders lie parallel to the axis of the driving shaft has always been a form of engine particularly attractive to aircraft designers. So far the mechanical difficulties involved in the transmission from the pistons to the driving shaft have been too great to permit of the adoption of this type. It is hoped, however, in the not far distant future these difficulties will be overcome. For the small engine, however, it is thought that such a design, incorporating an ordinary ball thrust race, as the wobble, or swashplate, could be quite satisfactorily designed; be capable of high speeds, giving low head resistance and reliability; and be comparatively cheap and easy to produce.

Four-Cylinder-in-Line.

It is curious that, though this type of engine is by far the most extensively used in motor-car practice, it has not come into prominence for light aircraft. As

an air-cooled engine, it is probable that owing to faulty cooling of the rear cylinders it would not be possible to obtain the high duty called for, unless some elaborate system of air scooping was fitted which would greatly increase the head resistance. Further, in its air-cooled form, the engine must be somewhat long owing to the necessity of arranging for the passage of air between the cylinders. An air-cooled engine, also, is dependent entirely for its rigidity on the crankcase, which is not wholly satisfactory. The other advantages of the four-cylinder engine are so well-known that it is unnecessary for me to deal with them here. In its water-cooled form, however, this type has much to recommend it both from a production and installation standpoint. As will be seen on reference to Fig. 1, it presents the smallest head resistance of any type. The outline of the engine shown in Fig. 1 is intended to be what is known in the Air Ministry as an inverted engine, *i.e.*, with the cylinders below the crankcase. It will doubtless be argued that a water-cooled engine is too complicated, when the radiator and water piping, etc., are taken into consideration. Further, that the additional weight due to these parts and the water would put this engine on a worse footing than air-cooled engines. In point of fact there is very little difference between the weight of a water and an air-cooled engine, for the weight of water and radiator is largely offset by the increased cylinder and piston weights of the air-cooled engine, and, also, to the lower speed at which it is necessary to run the latter owing to the fact that it is almost of necessity of the radial type, thereby introducing connecting-rod and crankshaft difficulties, which render the type unsuitable for running at high speed.

In the case of the light aircraft, it is considered that the advantage of the water-cooled engine would be even greater than with the large engine, owing to the fact that it would be possible to build in the radiators to the sides of the water jackets, thereby largely eliminating water piping. In the design diagrammatically indicated on the sketch it was intended that the radiators should be formed by two nests of " Brown " tubes, each a block of approximately 3 in. diameter and running the whole length of the crankcase on each side of the cylinder liners located at the highest point on the water space. From the point of view of weight, it is considered that, constructed with an aluminium cylinder block with steel liners, and otherwise conforming to standard high performance motor-car lines, a rigid and, consequently, sweet running engine could be produced at a weight certainly not exceeding that of the 180° twin.

Straight Six and Straight Eight Engines.

As we now have the luxury and speed model car, so we may expect in the future to see similar classes of light aeroplanes, and, for the latter, doubtless both the straight six and the straight eight will have strong supporters. The advantages claimed for the four-cylinder-in-line engine, with the exception of simplicity and first cost, apply to an even greater extent in the case of engines with the larger number of cylinders in line. As an instance of what can be done, the writer has recently been given the opportunity of considering a proposal for a 1,500 c.c. eight-cylinder-in-line design to give 70 B.H.P. continuously at a speed of 5,000 r.p.m. Owing to the good balance and slight torque fluctuation of these multi-cylinder straight-line engines, the machine designer's problem is greatly simplified, and engines of these sizes could quite easily be designed to be self-supporting from a flange mounting, thereby eliminating the necessity for the introduction of engine bearer weight, which presents somewhat of a difficulty in the case of the larger engines of these types.

(*e*) *Most Suitable Types for Various Duties.*

To sum up, the writer is of the opinion that it is not possible to exalt any one type of engine as eminently suitable for universal adoption on light aircraft of all types and for all duties. Light aircraft requirements will, it is thought, vary

widely in the future, and the engine builders must be prepared to meet all these requirements. The writer's suggestions as the most suitable types of engines for the various requirements as they can be foreseen to-day are as follow :—

Single-seater :	180° Twin Air-cooled.
Two-seater :	Five-cylinder Radial Air-cooled, or Four-cylinder-in-line Water-cooled.
Luxury or Speed Machine :	Six or Eight-cylinder-in-line, Water-cooled.

(f) Installation.

From an examination of the machines in the Lympne Competition, it is considered that the same attention is not given to the problem of satisfactorily installing the engine as is usually the case on aircraft of greater power. For instance, engine mountings in some cases gave the impression of having been designed at the time the engine was installed in the machine, having too many parts in the form of tubes connected by forked joints and bolts, or, in some cases, the ends of tubes just flattened out and bolted to the engine and fuselage. As already pointed out, a two-cylinder engine calls for great care in mounting, and, in this respect, the engine designer might have rendered considerably more assistance to the machine designer by supplying satisfactory bearers as part of the engine. This is now becoming standard practice for engines of over 400 h.p., and the engine designer's systems of engine supports are only arrived at after a considerable amount of discussion with the machine designer. The argument against the engine designer producing the engine mounting himself is that in some quarters it is held that this hampers machine design. In practice, however, this difficulty has not become apparent. The great advantage of the engine builder producing the mounting is that he can test his engine installed in the same way as it will have to run when fitted into the machine.

As regards cowling, it is preferable that this should also be carried out by the engine builder, as, if this is so, adequate provision for attachment will be provided on the engine itself, and, in consequence, one very serious source of trouble with machines in service would be avoided. The difficulty of introducing this, however, is far more serious than in the case of the engine mounting owing to the different body shapes of the various aircraft.

If light aircraft are to become popular, the importance of as rigid fire prevention as on larger aircraft must be realised. Fireproof bulkheads must be constructed with the same care and have no unbushed holes made for control rods, etc. Here, again, the engine designer can be of great assistance, as, if he makes provision on his engine for it to be bolted up to a flat plate, such as is formed by the fireproof bulkhead, and conveniently arranges his controls, the machine designer will have no difficulty in making a really sound fireproof job.

Engine builders have always been somewhat sensitive about the engine controls supplied by the aircraft designer, and these will require to be an especially good job on the light aircraft, where such fine control will be necessary especially on the altitude control. In this connection, it is pointed out that the Bowden engine control has been prohibited for years on Service aircraft, and there is no reason, therefore, why it should be considered good enough for light aeroplanes.

The importance of accessibility to the engine and its auxiliaries generally cannot be over-estimated, and, if the engine designer provides his own engine bearer, he has no excuse if his engine lacks merit in this respect when installed in the aeroplane.

Conclusion

In the writer's opinion, as is the case with all other aircraft, the successful development of light aircraft for any purpose whatever depends on the engine

designer. The competitions so far have indicated that the light aircraft engine builder's problem is a far more serious one than was anticipated. In this lecture the writer has indicated why this is so, and that it is only by the very best work of those most highly skilled in high efficiency internal combustion engine design that an engine sufficiently powerful, and, at the same time, durable, can be produced. The light aircraft engine must always be more expensive than any other engine of similar capacity, for the simple reason that, in addition to the fact that it will always be called upon to give the maximum duty obtainable from its capacity, as is the case with the racing motor car engine, it has the further disadvantage of having to be produced at minimum weight. It will no doubt be stated that the maximum possible rating will not be used, and that reliability shall be accepted in lieu. It has been the writer's experience, however, during the last ten years, if there is anything left in the way of power inside an engine, the aircraft designer will have it out, and he sees no reason why this is less likely to be the case with the light aircraft engine. It is absolutely essential that an aero engine be designed to withstand indefinitely the maximum duty which it is possible to obtain by running full throttle under the best possible settings for carburettor and ignition for maximum power.

Finally, the writer would like to say that in this lecture he does not pretend to have covered in any way the whole ground, nor would this be possible in the course of one lecture. He hopes, however, that, as a result of his remarks and the suggestions which are contained therein, a good discussion will ensue.

Discussion

Lieutenant-Colonel W. A. Bristow said that he had listened with great interest to the paper, and although it contained a great deal of technical matter of a highly controversial character, he proposed to comment only on one or two points of principle.

The establishment of the volumetric rating for light aeroplane engines was not the work of either the aircraft industry or of the Royal Aero Club, but originated with the competition held in 1923 for prizes given mainly by a newspaper, who imposed the condition that the engines should not exceed 750 c.c. capacity.

Proposals are now being discussed with a view to the abandonment of the volumetric rating for future competitions and the substitution of some sort of rating on the basis of the total weight of the aircraft. If these proposals were accepted, the design, capacity and output of the engine would be left entirely to the discretion of the constructor of the aircraft providing that the total weight with fuel for two hours did not exceed a certain limit.

The lecturer's criticism of the capacity rating seemed, however, to involve a contradiction, because although he began by expressing his objection to it, yet a little later was recommending the boosting of engines by supercharging in order to obtain the greatest possible output per unit of volume.

In his opinion, however, there was no case whatever for supercharging engines of this character unless it was required to obtain for some unusual purpose the greatest possible output from a given volume, as was the case with racing motor-car engines competing in events on a volumetric classification.

It was true that in this latter case makers had succeeded in obtaining a very considerable output per unit volume, but he disagreed with the lecturer that it would be possible to bring down the weight per h.p. of these engines and blowers to anything like the figure necessary for aircraft work. At present the weight per h.p. of the lightest of these engines was about eleven pounds, and even at this figure he very much doubted whether the engines could pass the Air Ministry type test.

Even then the lecturer had not made out a case for this type of engine for aircraft, because on his own figures it was giving only 4.6 h.p. per 100 c.c., whereas there were air-cooled cycle-engines to be found at Brooklands whose output sometimes reached a figure of 5 B.H.P. per 100 c.c. without any supercharger and running at little, if any, higher speed than the car engine.

He differed from the lecturer as to the causes of the high cost of production of these light aircraft engines; in his opinion it was not because they gave more horse-power than some light car engines, but because their low weight per h.p. necessarily demanded a much higher standard of material, design and construction, and also the engines were made in half-dozens as against thousands.

He was sorry to hear the lecturer at this date asking for two magnetos on account of the low factor of reliability of this unit. It was certainly true that two sparks per cylinder were almost essential for the production of maximum power combined with smooth running, at least in the great majority of aircraft engines, but where this object could be attained with one magneto and two sparks in service it seemed a retrograde step to insist upon two magnetos.

The obvious course in the case of these small engines where the weight of accessories was relatively so high would be to make the magneto as reliable as the other parts of the engine. This would be easier in a two sparks in series magneto if a little more space and weight were allowed in the armature.

As regards the relative weights of air- and water-cooled engines, surely the lecturer was in error in assuming that for any aircraft engines the weights in flying trim were even approximately similar.

On referring to the official figures of the Air Ministry type tests it would be found that the best air-cooled engined was in the region of 1.6 lbs. per h.p., and the best water-cooled engine about 2.6 lbs. per h.p.—a difference of over fifty per cent. There were other important factors, of course, in addition to the weight per h.p., but the statement as to the possibility of producing water-cooled engines for these small machines for the same weight per h.p. as air-cooled engines looked rather hopeless in the light of these figures.

Mr. W. O. MANNING was of opinion that if there was to be any development of the light aeroplane, there was no question whatever that the engine must be a cheap one, and there was no doubt that the cheapest type of engine one could get was one with air-cooled cylinders, and so far the air-cooled engine had been satisfactory.

He noted that the lecturer in the second paragraph of his paper had stated that he would give the figures for motor-cycle engines later on in the paper. They seemed to have been omitted. Although the lecturer had stated that it was not possible to fly satisfactorily with a motor-cycle engine, the Blackburne engine had given satisfactory service, having flown over 1,000 miles without trouble, and had been fitted on many machines. He believed that the A.B.C. engine used last year was quite satisfactory, and stood up well to its work.

With regard to altitude control, he had noticed that on the modern French Claudel carburettor a sliding sleeve over the diffuser was used. This appeared to be a cheap and good method that might be used if altitude control was necessary.

In his opinion, as regarded supercharging, he thought it would be better to increase the size of the cylinder and do away with the supercharger.

Apart from the question of weight, there was no question at all that the air-cooled engine was cheaper. The lecturer had recommended a five-cylinder radial and said that it would weigh no more than a three-cylinder, but later on had shown that as the cylinders decreased in size, the weight went up proportionately.

The four-cylinder-in-line engine had been used in France, and geared down it ran well, and was of interest to designers in many ways.

He raised the question of what was required from an engine for a two-seater. He suggested 50-60 horse-power, propeller speed 2,000 r.p.m. Whether the engine should be geared down or not was for the engine designer to decide. He would like this question more carefully studied so that designers could have authoritative information on it.

Mr. FEDDEN: I should like to congratulate Colonel Fell on his excellent analysis of light aeroplane engines; there are, however, certain suggested lines of development upon which I would like to make a few comments.

Method of Rating.

Colonel Fell states that there is no longer any reason to retain any restrictions on light aeroplane engine sizes. From an engine designer's point of view, cutting out all such restrictions is distinctly attractive, and from a service point of view I do not think the size of engine is of importance, but as I believe we must look to competitions and races as one of the chief incentives for light aeroplane engine development in the future, in spite of what Colonel Bristow has said, I submit it would be dangerous to do away with restrictions altogether, otherwise it is impossible to adequately compare performance. Cubic capacity may be the wrong way to restrict light aero engines, but I do not think it would be wise to abandon all restrictions, otherwise we shall have freak expensive machines winning competitions which are of no use to the general development of the light aeroplane.

Air v. Water-Cooling.

Under this heading Colonel Fell states that, in large sizes, air-cooled engines are not necessarily lighter or cheaper than water-cooled engines.

With this statement I cannot agree. Proved air-cooled engines exist to-day which are twenty-five per cent. lighter than the best water-cooled engines of corresponding power and performance, and it is this very important power-weight advantage, plus the considerable reduction in man hours to produce and overhaul an air-cooled engine, which I believe are mainly responsible for making such countries as U.S.A., Italy, etc., to turn their attention so seriously to air-cooled engines.

After hearing the latest commercial aviation reports on the troubles encountered with water-cooling on the very best large aero engines, I cannot but believe the author has cleverly included his intriguing inverted monobloc water-cooled engine simply to tempt us to argue against it. Water-cooling troubles are in no small measure due to a combination of natural periods, which conditions are more likely to occur on a small engine in a light aeroplane than in a normal machine.

I am greatly adverse to introducing water-cooled engines for light aeroplanes; also I estimate that a four-cylinder water-cooled engine would be twenty per cent. heavier and thirty per cent. more expensive than an air-cooled engine of corresponding power.

The problem of designing a big end bearing assembly for a small radial engine to run up to the limit of propeller efficiency is not a difficult one.

If the four-cylinder water-cooled inverted engine is to be run at really high speeds, it must be geared and must have a flywheel, when it will be nearer forty per cent. heavier than a corresponding air-cooled engine.

In his interesting silhouette diagram, I think the author has been rather unfair to the three-cylinder radial; I am sure a smaller diameter design could be evolved for a similar capacity.

Supercharging.

Although such types will sure to be used in the future on competition machines, I cannot see much practical value for the supercharger without an increase in revs., which brings in the gearing problem. I think supercharging should be left out, except as an interesting technical development, at any rate for the next few years.

Quality of Workmanship.

I quite agree with the author as regards the necessary quality of workmanship, and the difficulty of production of light aeroplane engines. I do not think the average engineer has any real conception of the class of workmanship and interchangeability demanded on the few types of large aero engines made in this country to-day.

Since this question of workmanship is just as important, or more important, for small engines, I submit simplicity is the keynote of future engine development, to keep the man hours of production as low as possible.

If the really light aeroplane for clubs and sporting flying is going to be a popular success, I submit we must concentrate on two cylinders only for the power unit in order to keep the power-weight ratio and man hours within reasonable figures.

For service machines, and two-seaters, I am informed by people who ought to know the final solution will be an aeroplane of heavier all-up weight, and a considerable reserve of power to those used at the last Lympne Competition; these machines will have engines nearer three litres' capacity than the 1,100 c.c. Even with this type of engine, I submit we must still watch weight and cost of manufacture very closely indeed, as these are vitally governing factors, and for an engine of this size giving 50 to 70 h.p. I would submit that a four-cylinder inverted air-cooled in-line engine, or three or five-cylinder air-cooled radial, are the most promising solutions.

Captain WILKINSON congratulated the lecturer on his paper, and was pleased to hear that it was proposed to have the minimum power, maximum weight and maximum propeller speeds given to the engine designer rather than volume. Volume might be useful for class racing for motor-cars, but was not necessarily suitable for aeroplanes.

Colonel Bristow had stated that in the new regulations being drawn up for the light aeroplane trials of this year, the engine would not be rated on volume. If new regulations were being prepared now, the engines could not be ready in time. At least two years should be allowed for an engine to be designed and built, and the experimental work necessary to eliminate teething troubles completed if the engine is to be of light weight and have sufficient reliability.

Mr. PARNACOTT confirmed the last speaker's remarks about rating on a weight basis.

He thought Colonel Fell had made it clear that motor-cycle engines generally were unsatisfactory for aircraft work; he considered that some encouragement might be given to manufacturers to produce aircraft engines by offering prizes for the more successful. Light cars were wanted as well as light planes; in both the costs were important; the engine which proved durable for the continuous big power output demanded of a plane engine would prove the engine for a really light car. An advantage of the air-cooled engine for aircraft work, and for light road vehicles, was that the air-cooled engine had been known to work after having had a bullet through one cylinder. With water-cooling, the vulnerability was far too great, and unnecessarily so. War as well as the poor are at the door.

Colonel Fell had spoken of the M.E.P. of the cylinders. It was important, from the power for a given fuel consumption point of view, that the products of combustion should be expanded more in the cylinder. Further, the noise would be less if the exhaust valves opened against less pressure.

He had had some experience of the swashplate engine, and thought experiments with these might be well rewarded if the flexing of the shaft carrying the swashplate were stiffer, were much thicker about the centre of the swashplate and tapered towards the thinner ends. As has been shown, this Michell engine may be in perfect running balance, but not if the shaft reflexes considerably; there is a necessary flexing when the engine is running fast due to the flywheel trying to get into a plane at right angles to the axis of rotation.

The lecturer quoted the Sunbeam racing engine of 1,500 c.c. as used at Brooklands. The speaker thought that to be successful with racing cars everything had to be considered from the point of view of winning the race. In fact, the proportions might be considered good if the car and engine broke down as soon as the race was over, having been designed to do just the job and no more. In the case of aircraft, however, durability and reliability were absolutely necessary, so that car-racing practice need not influence the aircraft engine designer at all.

Fire prevention.—In the report of the inquiry into the terrible accident at Croydon on Christmas Eve nothing was said about the machine catching fire; people were—naturally—very sensitive about the dangers of the aeroplane turning over on landing, and quite frightened away when to this is commonly added a conflagration. Known modifications might make flying really attractive to civilians.

Coupled with the facts that the carburettor and its float chamber are the chief fire risk, and that the high-compression engines of to-day so frequently want "decarbonising" to avoid pre-ignition, he would like to suggest the introduction of higher-compression engines in which the fuel was pumped in on compression, such as a Diesel. He knew that there were considerable difficulties, but had always found that if engineers were encouraged to do anything, they generally did it. The war proved the engineer.

Captain SAYERS gave some experiences with an A.B.C. two-cylinder engine of over 3,000 c.c. The torque recoil was colossal, but was successfully taken up on rubber buffers, and the same method could be used for less difficult cases.

He thought that the lecturer had started with the assumption that the light aircraft engine must be of the high efficiency type and very expensive. He thought that that on the contrary was the last type that would be used. Simplicity and reliability were wanted. If torque reaction could be overcome, as it could be, there was nothing to stop the use of engines with a few large cylinders.

Mr. LITTLE thanked the lecturer for his interesting paper, and drew attention to the advantages of the two-stroke engine for use in light aircraft. Among these advantages were good power/weight ratio, simplicity, low initial cost, and low cost of maintenance. Unfortunately, insufficient experience had been gained with the fitting of this type of engine to the machine before flying with it at Lympne last year, and it had been unable to complete the tests, so that he had not much data to give them, but what there was might be considered of interest. The engine developed 13 B.H.P. at 2,300 r.p.m., 15 B.H.P. at 2,300 being the maximum; weight, 36½ lbs. complete with mounting and carburettor and propeller hub. That gave 2¾lbs. per h.p.

Another point was the small amount of petrol it consumed, which compared very favourably with the consumption figures that had been given for motor-cycle engines. Unfortunately, no further experiments had been made with the two-stroke engine after the Lympne Competition, though he was of opinion that there

was a big field for it, with its cheapness, simplicity and good power/weight ratio. A considerable amount of experimental work had still to be done, but it would be well worth while to spend time and trouble to develop this engine.

Mr. ACRES agreed with a former speaker that the modern high-speed engine was an abomination. He had been much blamed for saying the same thing at the Empire Automobile Conference at Wembley, but it was undoubtedly a fact that as long as the capacity rating was retained for competition purposes, speeds and compression ratios were bound to increase.

There seemed rather an inconsistency in the paper, for at one time the lecturer condemned the capacity rating, and at another showed how to get the best out of it.

With regard to the supercharger, the author had quoted the Sunbeam racing cars, but these figures are not particularly convincing in view of the author's earlier expressed opinions, for they would appear to demonstrate that the use of the supercharger was merely to give higher speeds rather than any appreciable increase in brake mean pressure; for the figures given correspond to a B.M.E.P. of 109 in 1922 (not supercharged), 131 in 1923 (not supercharged, but such high compression that alcohol had to be used, not petrol), and 139 lbs. per sq. in. in 1924, with supercharger and reverting to petrol. The piston speeds were 2,770, 2,770 and 3,120 ft. per min. In a 1914 racing engine, for the design of which the speaker was responsible, he obtained a B.M.E.P. of 137 lbs. per sq. in. at piston speed 3,070 ft. per min. and 134 at 3,480. This engine was not supercharged, had only 5.4 to 1 compression ratio, and used straight petrol.

The author mentioned the flat four-cylinder at 180 degrees. In 1910 or 1911 the firm with which the speaker was then connected manufactured a flat four of this type for Captain de Havilland. That engine had one very nasty characteristic—the horizontal couple causing wobble. This same couple, coupled with inertia torque reaction, is also one of the troubles of the flat twin. Is this couple responsible for the limitation as to size? If so, it would apply equally to the flat four, for as the horizontal couple is balanced out, so the vertical couple increases. In the flat twin the cylinders can be got reasonably in line, but not so in the four.

In Table II. the lecturer stated that some of the engines in the Lympne trials had a B.H.P. per cubic inch nearly 1.8 times that of the Napier Lion. He should have pointed out, however, that this was only attained by very high speeds of revolution. If we compare the brake mean pressures obtained from the horse-power given in the table, we find that the Lion gives 123.5 lbs. per sq. in. at 2,200, the Condor 119 at 2,200, the Cherub 119 at 2,200 r.p.m., 121.5 at 3,200 and 106.5 at 4,010, and the Blackburne (that to which he referred) 128.5 at 2,200, 124.8 at 3,600, and 121.0 at 4,000.

In passing, there is apparently a slight error in the figures for the Cherub, for B.H.P. per sq. in. piston area divided by B.H.P. per cu. in. cylinder volume gives the stroke, and this works out to 3.73ins., 3.86ins. and 3.70ins. respectively in the three sets of figures given. Which is the correct figure?

Mr. MANNING said he remembered in the early days of aircraft that the Antoinette had a system of cooling by evaporation, there being an ordinary tank behind the engine containing only water which was circulated round the cylinder jackets by a water pump. When it boiled the steam was carried out to radiators on the sides of the fuselage and brought back to the tank by a separate water pump. The engine itself was unreliable compared with present-day standards, but the cooling system worked very well indeed.

Wing Commander CAVE-BROWNE-CAVE drew attention to the advantage of the water-cooled over the air-cooled engine in respect of head resistance and the clear view which it allowed the pilot.

He said that evaporative cooling of the engine, if practicable, would reduce the weight of a water-cooled engine and have many important advantages for aircraft. He understood that at any rate one American car was working on this principle, and he would be glad if any member of the Institution of Automobile Engineers could give information about the system.

Supercharging would be of great assistance to the two-stroke engine.

He thought the Royal Aeronautical Society had benefited greatly from this joint meeting with the Institution of Automobile Engineers. The engine was such a vital part of aircraft and there was so much knowledge and experience of similar engines available among Automobile Engineers that he hoped there might be many more joint meetings.

Mr. ERIC GNAPP said that he greatly appreciated Colonel Fell's paper, but was not quite convinced that the small water-cooled engine was as light as the air-cooled engine. As regards Colonel Fell's statement that the water-cooled engine could be produced to give considerably less head resistance than the air-cooled, could not a smaller air-cooled engine be accommodated inside the area of the cross-section of the fuselage in front of the pilot, and the head resistance then reduced by streamlining the cowling; if necessary, lengthening the crank-shaft (in the case of a radial engine) to attain this?

The amount of head resistance caused by the requisite cooling of the cylinder heads would be no greater than that of the radiator in the case of the water-cooled engine.

Mr. OSWALD drew attention to what he thought was a defect in engines of this class, and that was the failure to provide anything accessibly adequate, for more than one position of the engine, for the aeroplane designer to get hold of, to fix the engine in the machine. The engine maker provided, say, four bolts, which were perfectly adequate if the engine was central with the four longerons of the machine, but the height of the average light aeroplane was such that in some cases for propeller clearance it was desirable to mount the engine on the two top longerons. This immediately made the engine mounting difficult, because of the trouble in avoiding the carburettor, etc., in getting hold of the bolts and the necessity at the same time of making the mounting robust enough to stand torque reaction. There were several engine-mounting failures at Lympne last year. The blame fell on the aeroplane maker, but it was partly attributable to the engine maker for the reasons given above. There was no doubt the question of engine attachment ought to be more carefully considered by the engine maker.

Mr. CARR, speaking from the aeroplane constructor's point of view, thought that it could be estimated that up to date about £50,000 a year had been spent on light aircraft development, and about that amount, it seemed, was going to be spent on light aircraft engines in 1925. He believed that the aeroplane and engine designers, if given a free hand, would have solved the problem in one year.

Mr. CURTIS thought that insufficient consideration had been given to the claims of the lower-powered engines.

In Table I. below are given particulars of a few achievements of machines with engines of under 750 c.c.

He believed that by paying more attention to smaller planes and lower-powered engines, both increased engine and aerodynamic efficiency and reliability would be obtained. Their performances had so far been very promising; they had the advantage of handiness, low first cost, low cost of upkeep, and economy of fuel and oil.

He believed there was a fairly generally held opinion that 15/18 B.H.P. (max.) was required for a single-seater (although one speaker had mentioned 40 B.H.P.). As shown in Table II., the Douglas 600 c.c. engine easily met this

requirement. For a two-seater 1,100 c.c. should be sufficient, allowing 10/15 B.H.P. for cruising, with a maximum of about 40 B.H.P.

He also pointed out that it would be of considerable assistance to engine designers if they could be informed of the B.H.P. actually required by the various classes of machine.

He mentioned that in Germany a great deal of attention was being paid to engines of under 750 c.c. A 350 c.c. engine had been flown for a considerable time in a single-seater (maximum B.H.P. about 11), while a 750 c.c. engine had been used for a two-seater. He suggested that the use of, say, two Douglas 600 c.c. engines instead of one larger engine would be an instructive experiment, especially so as a scale model of a larger machine.

TABLE I.

LOW-POWER AEROPLANE ACHIEVEMENTS.

Achievement.	Plane.	Engine.		Pilot.
Endurance.				
Flight to Brussels 4hrs. 5mins. in the air, 50 m.p.h.	De Havilland D.H.53.	700 c.c.	Blackburne.	Cobham.
1,000 miles in one week.	Avro.	700 c.c.	Blackburne.	Hinkler.
(Direct airscrew drive). 300 miles in 6 hours.	De Havilland D.H.53.	750 c.c.	Douglas.	Hemming.
Only finisher in Kissingen Race.	Udet-Kolibri.	750 c.c.	Douglas.	Udet.
4hrs. 39mins.	Udet-Kolibri.	750 c.c.	Douglas.	Udet.
2hrs. 3mins.	Rotor Vogel.	350 c.c.	Douglas.	Baumer.
Economy.				
87½ m.p.g.	English Electric "Wren."	400 c.c.	A.B.C.	Longton.
87½ m.p.g.	Anec.	700 c.c.	Blackburne.	James.
65.7 m.p.g.	Handasyde.	750 c.c.	Douglas.	Raynham.
50 m.p.g.	De Havilland D.H.53.	700 c.c.	Blackburne.	Cobham.
1.7 galls. for 2hrs. 5mins.	Rotor Vogel.	350 c.c.	Douglas.	Baumer.
Speed.				
76.2 m.p.h.	Parnall Pixie.	750 c.c.	Douglas.	Macmillan.
81 m.p.h. Hendon.	Parnall Pixie.	750 c.c.	Douglas.	Macmillan.
90 m.p.h. Hendon for lap.	Parnall Pixie.	750 c.c.	Douglas.	Macmillan.
Altitude.				
14,400 feet.	Anec.	700 c.c.	Blackburne.	Piercey.
13,850 feet.	Avro.	500 c.c.	Douglas.	Hammersley.

TABLE II.

PARTICULARS OF DOUGLAS 600 C.C. ENGINE.

R.P.M.	B.H.P.	B.M.E.P.
3,000	17.1	124
3,400	19.6	126
3,500	20.2	125.5
4,000	22.5	123
4,400	24.3	120

(Froude Dynamometer.)

Major Mayo expressed his profound disagreement with the arguments of the last speaker. He thought that one thing had been clearly demonstrated in the last three years, and that was that the very small aeroplane and engine were quite useless from the practical point of view. Figures had been given of the flying that had been done with these small aeroplanes, but surely the criterion was this:—The movement for light aeroplanes was now three years old; it had begun with gliding and had gone on with light machines fitted with engines of capacity up to 750 c.c.s. and then 1,100 c.c.s. The time was going on, but how many aeroplanes of these types had been sold in England to the public? He suggested that the number was not one. He did not believe a single aeroplane had been bought or used for private purposes. The reason was that such aeroplanes were totally unsuitable for use by the general public.

It had been said that there was a consensus of opinion as to what horse-power was required, viz., 10-20 h.p. He suggested that a more suitable figure was 60 h.p. for a single-seater machine, and 80-90 for a two-seater, but admitted that it was possible with good designing to make a reasonably satisfactory two-seater with 60-70 h.p. There was no indication whatever that very small machines were wanted. When one analysed the capabilities of the very small machines it was found that they could just take the pilot and passenger, but had no reserve of power; the engine had to be run full out all the time. That was not good enough. What one wanted was a more powerful machine that would carry two people with reasonable baggage, and it must be of a robust and cheap construction, not costly to maintain, and with a reserve of power to cope with adverse winds. The problem was quite different from those of the military or commercial passenger machine, both of which had to be highly efficient machines. In the case of the light machine, one could afford to have a heavy engine, not highly efficient. It need not have a low air resistance. A machine that would cruise at a reasonable speed with a reliable engine that would go on indefinitely was what was wanted.

Another point was the question of simplicity; that was a matter of prime importance and made a very strong argument for the air-cooled engine, which cut out many complications for the owner-pilot. For example, it would relieve him of the irksome necessity of draining the water out when he brought the machine in during the winter months. There were many ways in which the air-cooled engine made for simplicity.

The lecturer had stated that it would be difficult to cool the air-cooled four-cylinder-in-line engine. He ventured to suggest that this was not the case, and reminded the lecturer of the 80 Renault and 90 R.A.F., both of which did enormous service during the war, and were eminently reliable. The R.A.F.4A twelve-cylinder engine with two blocks of six cylinders in line was also most reliable. The problem was slightly different for such Vee type engines because there was a space between the two banks of cylinders which was of assistance in driving air between the cylinders; but he submitted that the cowling necessary to provide for the cooling of a four-cylinder-in-line engine was quite simple. The four-cylinder-in-line engine had many advantages and should not be ruled out on the score of the difficulty he had quoted.

He congratulated the lecturer on his paper.

Colonel Fell said that it had interested him very much to observe that there was another point on which the Institution of Automobile Engineers and the Aeronautical Society were in agreement, and that was rating. He quite agreed with everything that had been said against rating on volume, but could not help asking how they were to do without it. It was the only way to express the volumetric efficiency.

With regard to cooling engines by evaporation, he had spent quite an amount of time and money only a few months ago in trying to get information

on this point from America, but without success. There were people who claimed to have cars running with engines cooled by this method, but it did not seem to be known to the engine firms.

Lieutenant-Colonel Bristow stated that the writer's criticism of the capacity rating involved a contradiction because the question of supercharging was introduced.

Later in the lecture the writer made it clear he was in favour of the twin-cylinder horizontally-opposed engine as a type, but, unless excessive speeds were resorted to, it was not possible to obtain an engine of this type which was sufficiently powerful for two-seater light aircraft. If, however, the charge density were increased, it would permit the use of this type of engine for two-seater aeroplanes without resorting to excessive speeds. It was pointed out in the lecture that increasing the cylinder dimensions of the two-cylinder 180-degree Vee has not been found to produce a satisfactory engine.

With regard to the suggestion that track motor-cycle engines existed giving 5 B.H.P. per 100 c.c., it was pointed out that these are provided with compression ratios which permit of the use of "doped" fuel only, and that it is essential that the light aircraft engine must run on ordinary aviation spirit obtainable anywhere in the country. As pointed out, this is possible where the additional power of a given cubic capacity is obtained by the use of a supercharger.

The writer was of the opinion that magnetos cannot yet be considered sufficiently reliable to permit the use of one only; further, it had definitely been proved that up to date the dual-spark magneto was even less reliable than the single-spark. This was only natural, as the dual-spark magneto had not been developed to the same extent as the single-spark.

As regards the relative weights of air- and water-cooled engines, as pointed out, the air-cooled engine was almost of necessity of the radial type, whereas no such limitation was placed on the design of the water-cooled engine. Owing to big-end troubles there was a definite limitation to the speed at which the air-cooled engine could be run, whereas high speeds, and consequently greater power, could be obtained from the water-cooled engine offsetting the additional weight of water and piping which were involved. The respective weights of the air- and water-cooled engines referred to as having passed the Air Ministry type tests were not a sound basis for comparison, as water-cooled engines were geared and the air-cooled engines ungeared. Further, the air-cooled engines were in a later stage of development than the water-cooled.

The point raised by Mr. Manning that as the cylinders decreased in size the weight went up proportionately, and that, therefore, the writer's statement that the five-cylinder radial would be lighter than the three-cylinder was inconsistent; the writer wished to point out that it was not proposed that the three-cylinder and five-cylinder engine should be of the same capacity; the criticism was not, therefore, applicable. Mr. Manning stated 50/60 h.p., at a propeller speed of 2,000 r.p.m., was required, and at the same time recommended a two-cylinder engine. Unless such an engine were supercharged, as suggested in the lecture, the writer was of the opinion that a satisfactory design to comply with these figures could not be obtained on account of the large cylinders which would be involved.

Mr. Fedden considered it dangerous to do away with restrictions altogether. The writer pointed out that the limitations imposed by laying down the minimum h.p., maximum weight and Air Ministry type test, should eliminate the freak engine.

With regard to the silhouette diagram, the three-cylinder radial outlined was taken from an existing engine.

Mr. Parnacott raised the question of vulnerability of water-cooled engines.

The writer did not anticipate that the light aircraft engine would have any application for military purposes over the lines, and that therefore this point should not arise.

The suggestion raised by Mr. Parnacott that a compression-ignition engine should be developed for light aircraft was interesting, but the writer thought until the larger aircraft engine—which involved less difficulty—had been fully developed, it would not be wise to consider the manufacture of so small an engine as represented by the light aircraft engine.

The writer thought that Mr. Sayers had rather misunderstood the point of view which the writer was endeavouring to put forward. The writer fully appreciates the disadvantages of the cost of the light aircraft engine, but, in order to fulfil the demands of the light aircraft design, the engine builder had no option in the matter and was forced to produce an engine giving a high output for its weight and size; and, in order to obtain reliability, perfection of manufacture was called for that made it impossible for such an engine to be really cheap. He agreed with Mr. Sayers that a considerable amount could be done in improvements to engine mountings so as to allow of the use of horizontally-opposed engines of larger capacity, but that the type of mounting suggested by Captain Sayers had not been found satisfactory in every case.

Mr. Little raised the question of the use of two-strokes for light aircraft. Though such an engine has obvious advantages, the writer was of the opinion that so far no two-stroke had been developed which would be capable of the duty at present called for from a light aircraft engine. There is no reason, however, that a two-stroke, with a separately-driven blower, should not be developed satisfactorily.

As regards fuel consumption, it has been the writer's experience that the low petrol consumptions on two-stroke engines were obtained at the expense of consumption of lubricating oil.

Mr. Acres raised the question as to what was the limit to which the flat twin could be built. The writer was not prepared to give any definite figure, as some good designer would be certain to come along and prove it wrong; on the other hand, it appeared to be a fact that no successful flat twin engine had been built of the capacity of over 1,500 c.c. to his knowledge, though Mr. Sayers draws attention to a 3,000 c.c., which, he stated, was satisfactory.

With regard to Mr. Gnapp's remarks on the straight-line air-cooled engine, it had been found in practice that the cowling necessary to introduce sufficient air to cool the rear cylinders rendered the head resistance of this type considerable.

The writer quite agreed with Mr. Oswald regarding his remarks on engine mountings.

Major Mayo drew attention to the ease with which the 80 h.p. Renault and 90 h.p. R.A.F. were cooled. It had been the writer's experience, during the period that he was responsible for the repair of several thousands of these engines, that in spite of the low rating of these engines, the cooling was inadequate, and considerable trouble, due to burnt pistons, cracked cylinders, burnt-out valves, etc., was experienced. To get high duty from an air-cooled cylinder, the greatest attention is necessary to the cooling problem.

Mr. KERR THOMAS said that as Wing Commander Cave-Browne-Cave had left, he was unable to thank him for the kind remarks he had made. He thought it was very desirable that more joint meetings should be held, and suggested that it might be possible to arrange for two during each Session. It would be of advantage to the Institution of Automobile Engineers, and he hoped to the Royal Aeronautical Society also.

A hearty vote of thanks was accorded to the lecturer.

Captain BARNWELL (*contributed*): I should like to state that contrary to what appears to be the consensus of opinion, I am very keen that the 1,100 c.c. two-cylinder engine should be adhered to for light aeroplanes. It appears to me that light two-seater machines with quite reasonable performance could be developed for an engine of this size only, and I feel that it is rather a retrograde step to say that because the machines so far built have proved slightly under-powered, that we should develop a larger engine. I consider that the 1,100 c.c. engine is about the largest practicable size for a two-cylinder, and I do feel that from the points of view of cheapness of production, and ease of maintenance, every effort should be made to adhere to the two-cylinder engine, and that the problem of producing an efficient combination of engine and aeroplane should be mainly one of aeroplane development.

VORTEX MOTION BEHIND A CIRCULAR CYLINDER

BY R. C. J. HOWLAND, M.A., M.SC.

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The study of aerodynamics is still largely empirical, and there seems little hope at present that the gap between theory and practice will be sensibly diminished by any means at present known to the applied mathematician. Yet the light that would, in all likelihood, be thrown on a number of practical problems by the complete solution of even a single case of turbulent motion would be very great. Of the problems that can be attempted, that of the flow of a fluid past a circular cylinder is likely to prove the least intractable.

In the case when the planes of flow are perpendicular to the axis of the cylinder careful observations of the motion have repeatedly been made. These have shown that vortices of approximately circular form are thrown off from the two sides. During the subsequent motion, each pair of vortices appears at one time to be practically stationary in symmetrical positions, after which it moves down the stream, another pair taking its place.

Föppl* investigated the positions of equilibrium of a vortex pair behind a cylinder in a stream, the liquid being treated as perfect. He showed that all the positions lie on a certain simple curve and that all the positions are unstable. The investigation was continued by Professor Levy,† who found that the actual positions of the vortices in a real fluid lie on Föppl's curve. Taking the coordinates of the vortices from a photograph of the fluid motion he proceeded to give them the same positions in the theoretical motion and to trace the streamlines, which then showed a very close correspondence with those actually observed.

It therefore seems legitimate to hope that further light may be thrown on the problem by investigations along the same lines, and the present paper is a first attempt at carrying the analysis further. Föppl's results are first verified. An attempt is then made to trace the actual paths of the vortices and it is shown that, corresponding to each position of equilibrium, there is a set of symmetrical paths of the same type. The boundary conditions are then investigated and it is found that these approach most nearly to a condition of no slip when the vortices occupy positions not far from those at which, in a real fluid, they are observed to rest. Finally, the force on the cylinder is compared with that observed.

The Equilibrium Positions

The cylinder being of the unit radius and the velocity of the stream at infinity being V parallel to the x-axis, the motion is given by the complex function.

$$w = -V(z + 1/z) + ik/2\pi \log (z - z_1)(z - 1/z_1)/(z - \bar{z}_1)(z - 1/\bar{z}_1) \quad . \quad (1)$$

where (see Fig. 1) $\left.\begin{aligned} z_1 &= x_1 + iy_1 \\ \bar{z}_1 &= x_1 - iy_1 \end{aligned}\right\}$ . . . . . (2)

The velocity components are given by

$$-u + iv = dw/dz = -V(1 - 1/z^2)$$
$$+ (ik/2\pi)\{1/(z - z_1) - 1/(z - 1/z_1) - 1/(z - \bar{z}_1) + 1/(z - 1/z_1)\} \quad . \quad (3)$$

* Münchener Sitzungsberichte, 1923.
† *Aero. Journal*, June, 1919.

To find the motion of the first vortex, the term $1/(z-z_1)$ is dropped and z is put equal to z_1 in the remainder. Separating real and imaginary parts, we then find

$$\left.\begin{aligned} u &= V\{1-(x^2-y^2)/r^4\}+(k/2\pi)\{y/(r^2-1)+1/2y-(r^2+1)\,y/[(r^2-1)^2+4y^2]\} \\ v &= -2Vxy/r^4+(k/2\pi)[-x/(r^2-1)+(r^2-1)\,x/\{(r^2-1)^2+4y^2\}] \end{aligned}\right\} \quad (4)$$

The equilibrium positions are obtained by putting $u=v=0$, and then eliminating the ratio V/k. The resulting equation easily reduces to

$$4r^2y^2=(r^2-1)^2 \text{ or } y=\pm\tfrac{1}{2}(r-1/r) \quad . \quad . \quad . \quad . \quad (5)$$

Any point on this curve is a possible position of equilibrium for one vortex if the other is symmetrically placed. Each position, however, demands a special value of V/k, which may be found from either of the equations (4) by putting u or v equal to 0. Simplifying the result by means of (5) we find

$$2\pi V/k=-r^5/(r^2+1)(r^2-1)^2 \quad . \quad . \quad . \quad . \quad (6)$$

where r refers to the position of either vortex. The observed position of equilibrium in a real fluid is $x=1.89$, $y=.67$.

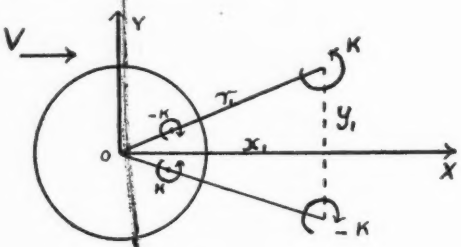


Fig. 1.

The Stability

If the two vortices are given small displacements, $\xi_1+i\eta_1$, $\xi_2-i\eta_2$, from their equilibrium positions, and their velocities then calculated, the following equations are obtained:—

$$\left.\begin{aligned} (2V/r^9)\ d\xi_1/dt &= A\xi_1+B\eta_1+C\xi_2+D\eta_2 \\ (2V/r^9)\ d\eta_1/dt &= E\xi_1+F\eta_1+G\xi_2+H\eta_2 \\ (2V/r^9)\ d\xi_2/dt &= C\xi_1+D\eta_1+A\xi_2+B\eta_2 \\ (2V/r^9)\ d\eta_2/dt &= G\xi_1+H\eta_1+E\xi_2+F\eta_2 \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (7)$$

where, after reduction by the use of (5) and (6), the constants have the values,

$$\left.\begin{aligned} &A=-F=2rx\,(3r^2-2),\quad B=2r^8+4r^6+r^4-5r^2+2, \\ &C=-H=2r^5x,\quad D=r^4(2r^4+r^2+1), \\ &E=2r^8-3r^4-5r^2+2,\quad G=-r^4(2r^4+3r^2-1). \end{aligned}\right\} \quad . \quad . \quad . \quad (8)$$

Using the relations $A+F=C+H=0$, the condition of stability is that

$$\lambda^4-2(BE+GD)\lambda^2+\{(A+C)^2+(B+D)(E+G)\}\{(A-C)^2+(B-D)(E-G)\}=0$$

should have all its roots purely imaginary.

It is thus required that

$$\left.\begin{aligned} &BE+GD<0 \\ &\{(A+C)^2+(B+D)(E+G)\}\{(A-C)^2+(B-D)(E-G)\}>0 \end{aligned}\right\} \quad . \quad (9)$$

On introducing the values of the constants, it is found that the first condition is satisfied. In the second expression, however, the first factor is negative and the second positive for all values of r. The positions of equilibrium are thus all

unstable. It is to be noted, however, that the condition of stability for a symmetrical disturbance is

$$(B+D)(E+G)+(A+C)^2<0$$

and this is always satisfied. It is thus to be expected that in the symmetrical motion, closed paths will surround each equilibrium position.

The Symmetrical Paths

If we write

$$X=-Vy(1-1/r^2)+(k/4\pi)\log\{[(r_1^4+1)-2(x^2-y^2)]^{\frac{1}{2}}/y(r^2-1)\} \quad (10)$$

equations (3) take the form

$$u=dx/dt=-\partial X/\partial y,\quad v=dy/dt=\partial X/\partial x$$

Hence

$$\partial X/\partial t=(\partial X/\partial x)(dx/dt)+(\partial X/\partial y)(dy/dt)=0$$

or X is constant with respect to the time.

The equation of the path of either vortex in symmetrical motion is therefore one of the set

$$[\{(r^2-1)^2+4y^2\}/y^2(r^2-1)^2]\,e^{my(1-1/r^2)}=C \quad . \quad . \quad . \quad (11)$$

or

$$\{1/y^2+4/(r^2-1)^2\}\,e^{my(1-1/r^2)}=C \quad . \quad . \quad . \quad (12)$$

where

$$m=-8\pi V/k=4r_1^5/(r_1^2+1)(r_1^2-1)^2 \quad . \quad . \quad . \quad (13)$$

and $(r_1,\ \pm y_1)$ are the positions at which vortices of strength k could remain at rest.

It is easily verified that, on the assumption that the vortices retain a constant small radius, (11) is an integral of energy. In fact, if ρ be the radius of each vortex, the variable part of the energy is proportional to

$$-(X+(k/2\pi)\log\tfrac{1}{2}\rho)$$

the constant of proportionality being positive.

Writing (12) as $F(x,\ y)=C$, and calculating F_x, F_y, F_{xx}, F_{xy}, F_{yy}, at the point of equilibrium we find, using (5) and (13),

$$F_x=F_y=0$$

$$(F_{xy})^2-F_{xx}F_{yy}=-\{16(3r_1^2-1)/(r_1^2-1)^4(r_1^2+1)^2\}\{(r_1^2+1)^2+4\} \quad (15)$$

which is certainly negative for points outside the cylinder. Every equilibrium position is thus surrounded by closed paths.

Again, the infinity points on (12) are given by the equation

$$(1/y^2)\,e^{my}=C \quad . \quad . \quad . \quad . \quad . \quad (16)$$

The function $z=(1/y^2)\,e^{my}$ has a minimum when $y=2/m$ and has no other turning values. Hence, for values of C less than $m^2e^2/4$ the infinity points are imaginary; when C has just this value they coincide; for greater values of C they are real and distinct and only two in number.

Corresponding to each equilibrium point there must therefore be a set of closed curves, surrounding each other and the equilibrium point, and themselves surrounded by a set of curves passing to infinity.

A systematic method of plotting the curves is obtained by writing the equation in the form

$$\log\{1/y^2+4/(r^2-1)^2\}=k-my(1-1/r^2) \quad . \quad . \quad . \quad (17)$$

For a given value of r the value of y is given by the intersection of the curve

$$Z=\log\{1/y^2+4/(r^2-1)^2\} \quad . \quad . \quad . \quad . \quad (18)$$

with the line

$$Z=k-m(1-1/r^2)\,y \quad . \quad . \quad . \quad . \quad . \quad (19)$$

From equation (18) a grid of curves is plotted, each for one constant value of r. These have now to be intersected by lines represented by (19), in which both k and m can be varied. If the gradients $m(1-1/r^2)$ are now tabulated for a given value of m, the intersections are readily found by drawing the lines on tracing paper for any value of k and placing this grid over the other; k may then be varied merely by moving the grid of the lines parallel to the $y-$axis. The paths for $m=1$ and for $m=6$ have been obtained in this manner. Those for $m=3$ were found by plotting the curves

$$Z=\{1/y^2+4/(r^2-1)^2\}\,\epsilon^{my(1-1/r^2)} \quad . \quad . \quad . \quad (20)$$

for different values of r and then cutting them by straight lines $Z=$constant. This method is more accurate than the other, but it requires a fresh set of curves for each value of m.

In each of the drawings (Fig. 2) the equilibrium curve (5) is shown as a broken line and the limiting curve, separating the closed from the infinite paths, is shown by a thicker line.

In the more general case in which the motions of the two vortices do not take place symmetrically, three integrals of the equations of motion are required for the determination of the paths. One integral can be obtained by the method of the present section, but the others do not appear to be easy to find. The problem should not, however, prove impossible of solution, and the curves obtained would be of much interest.

The Boundary Conditions

To find the tangential velocity at the surface $r=1$. Consider the product

$$i\,(dw/dz)\,\epsilon^{i\theta}$$

This is equal to

$$\begin{aligned}&-(iu+v)(\cos\theta+i\sin\theta)\\&=(v\cos\theta-u\sin\theta)+i\,(u\cos\theta+v\sin\theta)\\&=\text{velocity of slip}+i\,(\text{normal velocity})\\&=\text{velocity of slip.}\end{aligned}$$

Hence

$$q=-V\sin\theta + \{2k(r^2-1)\,y/\pi\}(\sin\theta)/\{[(r^2+1)^2-4y^2]-4(r^2+1)\,x\cos\theta+4r^2\cos^2\theta\} \quad (21)$$

x, y, r, referring to the position of the vortices.

The average velocity from $\theta=0$ to $\theta=\pi$ is

$$-2V/\pi-\{2k(r^2-1)\,y/\pi^2\}\int_{-1}^{1} du/\{4r^2u^2-4(r^2+1)\,xu+[(r^2+1)^2-4y^2]\}$$

which reduces to

$$-[2V/\pi+(k/\pi^2)\tan^{-1}2r/(r^2-1)]$$

or, giving k its value from (6),

$$(2V/\pi)[\{r^5/(r^2+1)(r^2-1)^2\}\tan^{-1}\{2r/(r^2-1)\}-1]$$

This will vanish if

$$\tan\{(r^2+1)(r^2-1)^2/r^5\}=2r^2/(r^2-1)$$

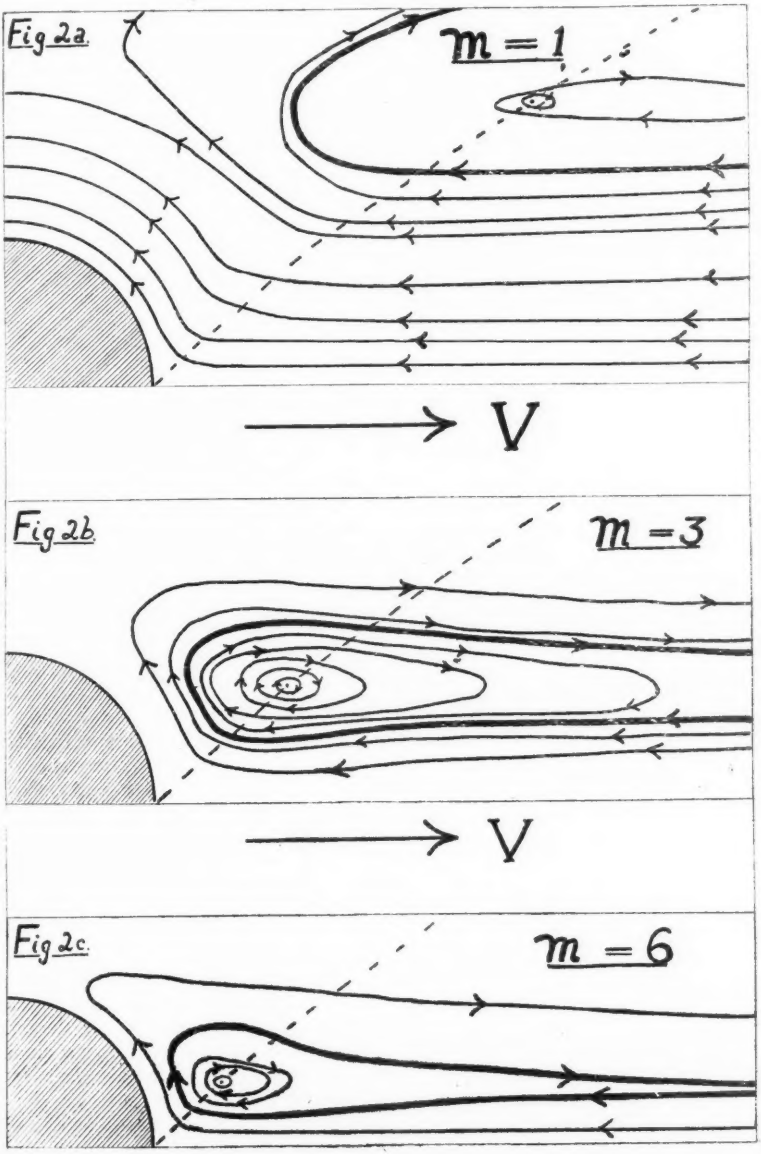
an equation which is readily solved graphically and gives $r=1.75$. This is not very far from the observed position of equilibrium, where $r=1.89$.

The Thrust on the Cylinder

The dynamical pressure at any point of the fluid is given by

$$\tfrac{1}{2}\rho\,(V^2 - q^2)$$

where, at the boundary, q is given by (21).



Taking the vortices in their observed positions, the pressure at any point of the boundary is readily calculated. The results are shown in Fig. 3A, in which the scale is taken so that $\rho V^2 = 1$. The distribution of pressure observed by Fage* is shown in Fig. 3B.

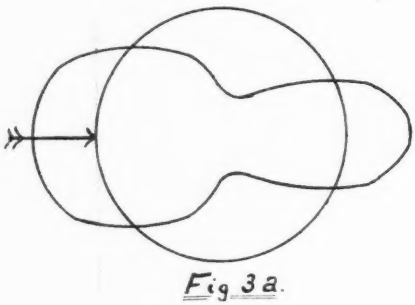


Fig 3a.

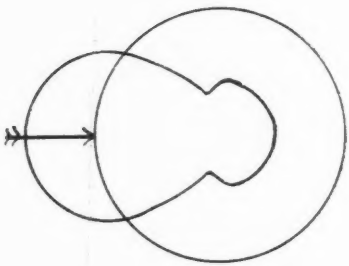


Fig 3b.

The pressure conditions behind the cylinder are very different from those observed, but it must be noted that in Fage's observations the pressures were averages over a period of time, while the configuration here supposed only occurred instantaneously.

By graphical integration the whole thrust on the cylinder may now be found. The results obtained are:—

Present theory $F/\rho V^2 = .3$ approx.
Observed $F/\rho V^2 = 1$ approx.
Irrotational theory $F/\rho V^2 = 0$.

From these and previous results it is evident that, in spite of the close imitation of the streamlines, the theory gives a very incomplete account of the motion. The reason for this may be sought for partly in the great difference of boundary conditions and an improvement might be made by introducing a modification at the boundary. But the assumption of steady motion must also be a source of error, the actual motion being periodic in character.

* Fage, *The Aeroplane* (Griffin, 1918).

Vortex Motion Behind a Cylinder

NOTE ADDED FEBRUARY 2ND, 1925.

The meaning of the very exact likeness of the observed with the calculated streamlines is made clear by the following simple theorem.

In a steady rotational motion, in two dimensions, the stream function satisfies the equation

$$\nabla_1^2\psi \equiv \partial^2\psi/\partial x^2 + \partial^2\psi/\partial y^2 = f(\psi) \quad . \quad . \quad . \quad . \quad (1)$$

when $f(\psi)$ is any function. This expresses the fact that the vorticity is constant along each streamline.

If a second motion has the same streamlines and a stream function ψ', we must have

$$X(\psi) = \psi' \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

and

$$\nabla_1^2\psi' = X'(\psi)\ \nabla_1^2\psi + X''(\psi)\ \{ (\partial\psi/\partial x)^2 + (\partial\psi/\partial y)^2 \} \quad . \quad . \quad (3)$$

The second motion can only be steady if the right hand side of (3) is a function of ψ', and hence of ψ, only. This will be the case if

(*a*) $X''(\psi) = 0$ and $\psi' = A\psi + B$,

or

(*b*) $(\partial\psi/\partial x)^2 + (\partial\psi/\partial y)^2$ is a function of ψ only.

The first condition requires that the second motion should be merely a repetition of the first with the velocities changed proportionally. The condition (*b*) requires that the velocity in either motion should be constant along each streamline, in which case the streamlines form a set of parallel curves.

In the case of the cylinder and vortex pair, the streamlines tend to become parallel only in the neighbourhood of the two vortices and in an extremely narrow band round the cylinder itself. A slight spreading of the vortices could have little effect on the motion, and the difference between the theoretical and observed motions is therefore to be sought in the region of laminar flow close to the surface. This conclusion is in entire agreement with recent work on aerofoil theory.

The proof of the above theorem makes certain assumptions as to the continuity of the functions involved. These assumptions are certainly justified if the fluid is regarded as having slight viscosity.

REVIEW

The Measurement of Fluid Velocity and Pressure

By J. R. Pannell. Edited by R. A. Frazer. (Arnold, London, 1924. Pp. VII. + 135. 10s. 6d.)

One cannot approach this book without remembering the catastrophe in which the author met his death. After reading it, one concludes that there could be no more fitting memorial to his life work in aeronautics. Those of us to whom Pannell was personally known will treasure the book on this account. But the author's large experience in the subject of which he writes, and the critical faculty he possessed in so happy a measure, have resulted in a work of a wide appeal.

With the exception of the hot-wire method, the fundamental ideas utilised in the measurement of fluid velocity are not recent, but it remained to aeronautics to establish precision instruments capable of determining the magnitude of the mean velocity over a small area of a free stream to within a few parts in a thousand and its direction to within a few hundredths of one degree. This achievement, for which we are indebted to the National Physical Laboratory, forms part of the backbone of aerodynamics, and a student, suddenly confronted with the science in its present highly developed state, could hardly do better as a first step than study it. More recently, many specialised devices for measuring airflow have been investigated on which the book contains intimate notes.

The treatment is essentially practical, while sufficient of theory is given or indicated for a sound grasp of the subject. In addition to sections on pressure-tube and moving-part instruments, there are adequate accounts of the less familiar hot-wire method and of manometers that should be particularly useful. The final chapter is very suitably devoted to a description of the important experiments on flow in pipes, in which the author collaborated with Dr. Stanton, and the book ends with references to difficulties still remaining in the exploration of velocity that present research has to face.

The book is profusely illustrated and the excellent presentation probably owes much to the able editorship of Mr. Frazer. It can be confidently recommended.